THE JOURNAL

OF THE

ROYAL AERONAUTICAL SOCIETY

(FOUNDED 1897 in succession to the ANNUAL REPORTS)

Edited for the Council by J. LAURENCE PRITCHARD, Fellow

No. 170 FEBRUARY, 1925 VOL. XXIX

NOTICES

Election of Members

The following Members were elected at a Council meeting held on Tuesday, December 13th:—

Associate Fellow.—Mr. C. A. Wright.

Students.—Mr. I. Levy and Mr. A. J. Newport.

Member.—Mr. A. G. Linney.

Usborne Memorial Fund

The Council desire gratefully to acknowledge a donation of £13 to the above fund from Sir Mackenzie Chalmers.

R.38 Memorial Fund

The Council desire gratefully to acknowledge a donation of 25 guineas to the above fund from Mr. C. P. Burgess, Commander J. C. Hunsaker and Mr. Starr Truscott.

The R.38 Memorial Tablet is now nearly completed and will shortly be fixed above the fireplace on the north wall of the Library.

Associate Fellowship Qualifications

The Council desire to call attention to the statement published elsewhere in this issue regarding the qualifications required from applicants for Associate Fellowship.

Cambridge University Aeronautical Society

The following programme for Lent Term has just been received from the Honorary Secretary, C.U.Ae.S.:—

Jan. 21st, 8.30 p.m.—Prof. B. Melville Jones, "Flying within Clouds."

Jan. 28th, 8.30 p.m.—Prof. L. Bairstow, "The Aerofoil: A Search for a Theory."

Feb. 4th, 8.30 p.m.—Wing Comdr. Sholto Douglas, "Air Fighting."

Feb. 11th, 8.30 p.m.—W. S. Farren, Esq., "The Process of Aeroplane Design."

Feb. 18th, 8.30 p.m.—Air Chief Marshal Sir Hugh Trenchard. (Subject to be announced later.)

Feb. 25th, 8.30 p.m.—H. Ricardo, Esq. (Subject to be announced later.)

March 4th, 8.30 p.m.—A. E. L. Chorlton, Esq., "All Steel Aircraft."

March 11th, 8.30 p.m.—G. Bradshaw, Esq., "The Failure of the Petrol Engine as a Prime Mover."

Scottish Branch

The following programme has been arranged by the Scottish Branch for the remainder of the present Session :—

Jan. 28th, 8.0 p.m.—Mr. C. G. Grey, "Aircraft in the Next War."

Feb. 16th, 8.0 p.m.—Mr. G. F. Luke, "The Beneficent Effects Conferred on Civilisation by Aviation Apart from the Carrying of Goods and Passengers."

Feb. 23rd, 8.0 p.m.—Mr. A. J. Cobham, "My Journey to India with Sir Sefton Brancker."

March 3rd, 8.0 p.m.—Mr. A. E. Hagg, "Control at Low Speeds."

April 3rd, 8.0 p.m.—Major G. H. Scott, "The New Empire View of Airships and the Practical Possibilities Arising Therefrom."

Lectures

It is regretted that Colonel Searle has again found it necessary to postpone his lecture on "The Maintenance of Commercial Aircraft." It is hoped to arrange a date in April.

Light refreshments will be served at 6.30 p.m. at the Royal Society of Arts before Colonel Fell's lecture on "Light Aeroplane Engine Development" on the 19th.

Forthcoming Arrangements

Thursday, Feb. 5th, 5.30 p.m.—Air Commodore C. R. Samson, "The Operation of Flying Boats in the Mediterranean."

Tuesday, Feb. 10th, 5.30 p.m.—Council Meeting.

Thursday, Feb. 19th, **7.0 p.m.—At the Society of Arts,** Lieut.-Colonel L. F. R. Fell, "Light Aeroplane Engine Development." Joint Meeting with Institution of Automobile Engineers.

Thursday, March 5th, 5.30 p.m.—Lieut.-Colonel C. B. Heald, "Some Medical Aspects of Air Transport."

Thursday, March 19th, 5.30 p.m.—Captain F. Tymms, "The Practical Navigation of Aircraft."

Thursday, March 26th, 5.30 p.m.—**At the Society of Arts,** Dr. Eckener, "Modern Zeppelin Airships."

W. Lockwood Marsh, *Secretary.*

ROYAL AERONAUTICAL SOCIETY

In view of doubts which appear to exist as to the qualifications required from candidates for Associate Fellowship of the Royal Aeronautical Society, the Council consider it advisable to issue the following statement as to their interpretation of the regulations.

The regulations provide for the election to Associate Fellowship of :—

(*a*) Those who pass the Society's own examination, or possess an equivalent University or Technical College qualification, followed by two years' practical experience in the application of the science of aeronautics.

(*b*) Those who present a thesis.

(*c*) Those who have very exceptional qualifications or achievements but do not possess the examination qualifications outlined in (*a*).

In regard to (*a*) the Council accept the degree or diploma of any recognised University or Technical College at home or abroad, and excuse from the Aerodynamics paper of the Society's own examination holders of Diplomas in Aeronautics of such Universities or Technical Colleges.

In regard to (*b*) the Council do not insist upon a thesis necessarily being a record of original work, but require it to be of such a nature as to give an adequate idea of the candidate's knowledge and experience in aeronautics or an allied subject applied to aeronautics.

Under the heading (*c*) the Council have accepted pilots of rigid airships, and aeroplane test and experimental pilots whose work has been, or is likely to be, of value to the progress of technical development. They also have accepted long-standing workers in any branch of aeronautics whose experience entitles them to be considered as "pioneers." In considering applications under this heading it is not possible to lay down any hard and fast rule, but each case must be considered individually on its merits.

The Council have also decided to publish the following correspondence bearing on the subject :—

11th November, 1924.

TO THE SECRETARY,
ROYAL AERONAUTICAL SOCIETY.

DEAR SIR,—I have to thank you for your letter of the 17th inst. containing an invitation to become a member of your Society. In reply, might I put before you a case which is not peculiar but common and will explain why I am not a member of that Society.

In the aircraft industry to-day there are a number of men of not inconsiderable experience . . . who have not attained to positions of any great responsibility but on whom, as section leaders and senior draughtsmen, the designer must depend to a large extent for the detail design and in a large number of cases the preliminary designs and performance calculations. Most of these men could and would design a machine should the opportunity present itself.

When, in its younger days, the Society offered to them facilities for becoming Associate Fellows, these men, for a multiplicity of reasons, let the opportunity slip. . . . Now that we would become Associate Fellows we find our way barred by an entrance examination which, to a man who has left school or college 15 or 20 years, appears quite formidable in its subjects. This same examination, however, permits the youth leaving college and having studied for three years

with this end in view to become an Associate Fellow. When it comes down to bed rock, the youth works under such men as those of whom I write and it will be years (judging from my experience) before this same youth will be able to occupy the section leader's position.

In other words, the Society is, at present, ignoring the men who have acquired their knowledge from experience and supporting the theorist who has gained his knowledge from his tutor. . . .

Yours faithfully,

ROYAL AERONAUTICAL SOCIETY,
7, ALBEMARLE STREET, LONDON, W.1,
21st November, 1924.

DEAR SIR,—I beg to thank you for your letter which raises an interesting question and which I should like, with your permission, to lay before the Council.

In the meantime, however, I should like to make one or two observations personally. In the first place it is not quite accurate, I think, to say that a youth leaving college with purely theoretical qualifications is at once entitled to become an Associate Fellow, as the Council insist upon a further qualification of two years' practical experience.

Secondly, the Council have the power in exceptional cases to accept as Associate Fellows individuals who do not possess the necessary exempting qualifications without calling upon them to pass the examination; while there is, of course, the further alternative of the presentation of a thesis.

These points, at any rate to some extent, deal with the grievance you suggest, and of course the Council always view with considerable sympathy applications from people such as those you write about, who have been engaged upon aeronautics for a long time and therefore come within the category of Pioneers. On the other hand, the aim is to raise Associate Fellowship to the same standard as other bodies, such as the Institution of Civil Engineers, and therefore it is quite obvious that considerable technical qualifications and/or experience must be insisted on.

Yours faithfully,
(Signed) W. LOCKWOOD MARSH,
Secretary.

23rd November, 1924.

TO THE SECRETARY,
ROYAL AERONAUTICAL SOCIETY.

DEAR SIR,—Your letter and enclosure to hand and for which I thank you. . . .

I read with interest your remarks *re* qualifications and must admit that I forgot the extra qualification of two years' experience. This does not, I think, materially alter the case I put forward.

Taking the thesis qualification, I should say that it could not apply to such men as those of whom I wrote. A thesis, as I regard it, to be of any practical use, must be a mathematical exposition of some more or less advanced theory on which the writer of the thesis has carried out some special research work. You will, of course, agree with me that such is impossible to us, whose duty it is, not to form and prove theories, but rather to be able to apply the results of other people's experiments to the actual machine. It is in this way that the Society is of such great use to us—we are able to get the results of experiments carried out by abler men and be prepared to apply them when necessary. Many

points are cleared up in the following discussions which would otherwise be left rather vague if the paper is merely read at home.

Our case then must be the claim of "Pioneers." . . . If your Council could be prevailed upon to give some definite length of experience as representing that of a pioneer then the whole case would be perfectly clear.

I hope I am not asking too much in hoping that you will be good enough to acquaint me with the decision of your Council before completing my proposal form.

Yours faithfully,

ROYAL AERONAUTICAL SOCIETY,
7, ALBEMARLE STREET, LONDON, W.1,
10th December, 1924.

DEAR SIR,—In further reference to my previous correspondence with you, I laid this before the Council at their meeting yesterday. . . . The Council confirmed the views expressed in my letter of the 21st November, but desired me to point out with reference to your letter of the 23rd November that you take a somewhat too narrow view of the type of thesis required. It is not at all necessary that such a thesis should be either mathematical or an account of some special research work. All that is required is such a paper as will enable them to judge the qualifications of the writer, and this might take the form, for example, of an account of the method of application of (to use your own words) "the results of other people's experiments to the actual machine."

They also wished me to say that they think it very possible that if such people as you refer to would fill in application forms they would prove acceptable as Associate Fellows; but that, of course, any individual case of an application unaccompanied by any examination qualifications or a thesis would necessarily have to be considered on its own special merits.

I may also say that the Council propose to issue a statement early in the new year explaining in detail the manner in which they interpret the regulations, somewhat on the lines of our correspondence.

Yours faithfully,
(Signed) W. LOCKWOOD MARSH,
Secretary.

PROCEEDINGS

SIXTH MEETING, 60TH SESSION.

A meeting of the Society was held in the Library at 7, Albemarle Street, London, on Thursday, December 18th, 1924, Lieutenant-Colonel H. T. Tizard, Chairman of the Society, presiding.

Colonel TIZARD, in introducing Mr. R. A. Watson Watt, said that the subject of his lecture was not only of immediate practical importance, but of even greater potential value.

THE BENEFICENCE OF ATMOSPHERICS

BY R. A. WATSON WATT.

It has frequently been remarked that the study of the naturally occurring electro-magnetic waves, which we now call atmospherics, is older than is commercial radiotelegraphy. It is, however, only within recent years that the development of radiotelegraphy and its allied sciences has allowed us to advance beyond crudely quantitative studies of these waves. In selecting for the consideration of this Society certain portions of recent work, I have been guided by the very special interest which those who go up to the air in ships must have in two very diverse aspects of atmospherics. The airman is one who is peculiarly and respectfully interested in thunderstorms and their dangers; he is also, in view of the three-dimensional freedom and the speed of his movements, peculiarly dependent on radiotelegraphic communications.

Now the atmospheric has a sort of Jekyll and Hyde duality which corresponds to these two interests; its maleficent influence on communications has been widely discussed and is undoubtedly the most serious remaining limitation on radiotelegraphic signalling; but it has also a field for benevolent assistance to the pilot and navigator which has not yet come into sufficient prominence. The atmospheric is a long distance, instantaneous and automatic signal of thunderstorm conditions, and I hope to bring you to share my belief that it may be made a very valuable accessory in the application of the "safety first" doctrine enunciated by Dr. Simpson in his recent lecture on thunderstorms. I propose, therefore, to devote special attention this evening to that aspect of the atmospheric in which it is to be regarded simply as a meteorological phenomenon, and to investigate the possibility of basing on its indications a thunderstorm warning service for the guidance of the aerial navigator who desires to steer clear of thunderstorms.

If we are to contemplate basing such a service on observations of atmospherics, we must establish the general validity of the postulates underlying the proposal. These are:—

(*a*) That a recognisable type of atmospheric, if not all atmospherics, can be shown to be produced by discharges of lightning character.

(*b*) That means can be provided for the location, with a degree of accuracy to be specified by the navigator, of the nearest source of such atmospherics, *i.e.*, of the nearest thunderstorm.

(*c*) That this location can be carried out over margins of time and space sufficient for the avoidance of the storm by any necessary change of course.

(*d*) That the method shall be applicable to any storm sufficiently developed to be dangerous to aircraft.

(*e*) That it shall not be such as to "cry wolf" in any large percentage of cases in which danger is in fact unlikely.

I may at once say that I do not pretend that on this day of this year of grace I am to hand you a recipe for a service completely fulfilling these requirements, but I do propose to show you how the evidence now available points towards their fulfilment.

It has from the earliest days of radiotelegraphy been clear that the strongest atmospherics received at any station always proceeded from lightning at no great distance. In 1895 Popoff used a coherer recorder of atmospherics as a lightning recorder. In 1902 Captain (now Sir Henry) Jackson called attention to the relation between atmospherics and Mediterranean depressions. The question whether all atmospherics originate in disturbances which a meteorologist would, without adopting a revised definition of the term, call thunderstorms is still, however, an open one.

I have made a somewhat detailed though still incomplete study of the only data which appear to me to be at all adequate, or rather which do not appear to be grossly and hopelessly inadequate for a discussion of the question. The only methods which have, until quite recently, been available for investigations of this kind have been those of direct aural reception and the application of the ordinary radiogoniometer or direction-finder to determine the azimuth from which atmospherics appear to be arriving at any selected place and time. The first systematic observations of this kind were initiated by Captain C. J. P. Cave, M.A., in 1915.* Within the last few years various observers† have used isolated direction-finders for limited periods in a research of the meteorological parentage of atmospherics, but it is scarcely necessary for me to point out that such methods, apart from inadequate sampling in time, do not purport to do more than select the great circle on which lie the observing station and the source of atmospherics, and that it is only necessary to proceed far enough along a great circle to meet conditions to support almost any thesis. I must confess that I regard all such limited tests as quite inadequate to establish a hypothesis, however valuable they may be as supports to a more rigorous investigation.

The more rigorous investigation must be based on the use of a group of direction-finders, well distributed on suitably long bases, whose observations of the apparent direction of arrival of atmospherics can be combined on a gnomonic chart to give a determination of the apparent source of the predominant atmospherics at a given time. We were fortunate in being able to obtain data from such a group of direction-finders, arranged around the British coasts, and I propose to summarise the results of observations for two years, 1916-18.

In these two years there were a thousand occasions on which three or more stations, in the intervals of their war duties, found time and conditions enabling them to observe a direction of arrival of atmospherics within the same hour. In four hundred and ninety cases these approximately simultaneous bearings converged to meet in a point or to delimit a very small area indicating the apparent place of origin of the atmospherics. The meteorological records from these regions have been examined in some detail, and the results are set out in Table I., showing the number of apparent sources of atmospherics located in various regions between April, 1916, and April, 1918, with the percentage number of such locations which have been identified as associated with

* Cave and Watt, "Study of Radiotelegraphic Atmospherics in Relation to Meteorology," *Q.J.Roy. Met. Soc.*, 49, 35 (1923).

† *e.g.*, Lacoste, Rothé.

(*a*) Thunderstorm phenomena within 250 kilometres.
(*b*) " " " 1000 "
(*c*) Hail showers within some 200 kilometres.
(*d*) " Passing showers " within a similar distance.
(*e*) Squalls.
(*f*) Rainfall.

The allotment of any individual case to one column implies the absence of reported phenomena belonging to earlier columns—otherwise expressed, the cases are allotted to the highest degree of " thunderiness " reported. The inadequacy of the criteria is clear; hail, passing showers and squalls have not yet been limited to meaning the same things for all observers; and all the criteria adopted, save lightning, are merely collateral effects, and not causes, of the phenomenon of electro-magnetic radiation. But in the default of a closer study of the physical processes the classification may serve. It is supplemented by a column in which, when no thunderstorm, squall or precipitation phenomena have been traced, the occurrence of barometric minima, troughs of secondary and V-shaped depressions, and marked local irregularities of isobars near the time and place of apparent origin of atmospherics, has been noted. The re-study of the whole of the data on the lines of this column, or preferably with reference to the " cold fronts " and " warm fronts " with which isobaric irregularities are associated, would be interesting. Meanwhile, the fact that a meteorological correlation of sorts has been found in all save 3 per cent. of the British cases, and, with very inadequate data, in 87 per cent. of all cases is significant, especially in view of the comparatively large number of locations, averaging two per three days during the period.

*[It will be noted that the detailed records of the closest and best organised meteorological networks of Europe are remarkably concordant in showing that only one-third of the locations obtained are associated with reports of thunderstorms near the apparent source of atmospherics, but that other meteorological correlations can be obtained in nine cases out of ten.]

The mean distance between an apparent source and the corresponding " near " reported thunderstorm is approximately 100 kilometres, the corresponding value for " distant " thunderstorms is between 300 and 400 kilometres. Distances over 500 kilometres are rarely used save in regions where data is insufficient—*e.g.*, Italian thunderstorms have had to be used in considering North African sources. It should be noted that the distance between apparent source and reported storm is not to be regarded as a measure of error in location, the storm is merely the nearest reported index of conditions in the vicinity of the source. In 21 cases the location fell within 20 kilometres, and in 44 within 50 kilometres of a reported thunderstorm.

The interpretation of these relations presents difficulties which cannot be discussed with any completeness on this occasion. How much the figures in the thunderstorm columns would have grown had all observers watched as carefully for lightning and thunder as did some, how many thunderstorms slip unobserved through the open mesh of the meteorological network, how far we may assume that cloud-to-cloud discharges of considerable magnitude are occurring over regions meteorologically disturbed, but formerly considered to be electrically quiet, why such discharges are not detected from the ground; all these questions must be left to meteorologists more competent and better informed than myself, or, in default of an immediate answer, to closer study by methods such as I have outlined.

But if we accept the results for the British Isles as a fair test of the conditions, the results affect the aerial navigator in this way. A network of direction-

* Throughout the paper, paragraphs enclosed in square brackets have been added in amplification of the original text, in order to deal with matters, arising in discussion, which can more conveniently be read in their proper sequence in the text.

TABLE I.

METEOROLOGICAL CONDITIONS NEAR APPARENT SOURCES OF ATMOSPHERICS.

Region.	No. of locations.	Thunderstorms within 250 k. %	Thunderstorms within 1,000 k. %	Hail. %	Passing showers. %	Squalls. %	Rain. %	Isobaric minima, &c. %	No correlation found. %
England and Wales ...	58	35	17	5	14	14	7	6	2
Ireland	45	13	13	2	44	16	8	0	4
Scotland	17	0	29	18	35	6	6	0	6
British Isles*	120	22	18	6	28	13	7	3	3
English Channel ...	58	31	7	2	20	9	15	7	9
France	146	36	4	2	1	1	27	13	16
Spain	23	13	—	—	—	—	52	13	22
Italy	19	42	—	—	—	—	26	21	11
Holland	7	86	14	—	—	—	—	—	0
S.W. Europe* ...	373	30	9	3	13	6	20	9	10
Central & S.E. Europe	12	33	8	—	—	—	—	34	25
Bay of Biscay... ...	38	—	11	3	—	—	60	3	24
Iceland and Atlantic ...	10	—	10	—	—	—	10	50	30
Belgium	7	29	43	—	—	—	—	—	29
Switzerland	6	33	—	—	—	—	—	17	50
Mediterranean and N. Africa	44	2	27	—	—	—	11	46	14
Total ...	490	25	11	2	10	5	21	13	13

finding stations, such as that actually used, observing atmospherics only as a very minor "side show" amidst pressing war duties, would have issued thunderstorm warnings on 120 occasions in the two years, or on the average just over one warning per week. Forty per cent. of such warnings would have been justified by subsequent reports of thunderstorms, a further six per cent. would have been justified by reports of hail, so that roughly fifty per cent. of the warnings would have been valid warnings of considerable dangers. The remaining fifty per cent. would almost all have referred to conditions in which cautious navigation would still have been necessary; not more than three per cent. would have been completely "false alarms" so far as meteorological disturbance of some sort is concerned, though many more would have referred to flying conditions which the pilot, given the warning, would decide to be comparatively safe. We are not justified, in the conditions of the test, in considering the thunderstorms missed by the network, since the proportion of the whole day's work which the stations could devote to "X-spotting" was very small indeed; their successes are astonishing in view of this fact and of the difficulty of direction-finding on atmospherics.

[A plain unqualified warning, without a specification of the relative intensity of the meteorological disturbance to be expected, would be better than no warning at all, but would tend to be neglected after a period in which slight disturbance only was found to have been the basis of the warning. It is therefore very desirable that the hypothetical system should have a means of assigning varying weights to the warnings issued. There is a very wide field for research into the interpretation, in relation to the meteorological circumstances of their origin,

* These lines represent sums from preceding regions, the three apparently arbitrary groups thus constituted being based on consultation of meteorological data of decreasing adequacy.

of the varying types of atmospherics received. This field may be explored by methods which have been developed more recently than those used in the 1916-18 work. In that investigation the data were not sufficient to enable us to decide the extent to which the magnitude of the meteorological disturbance could be inferred from these observations, but later instruments and methods are such that I believe an *ad hoc* network would give data of considerable precision as to the relative gravity of the warning.]

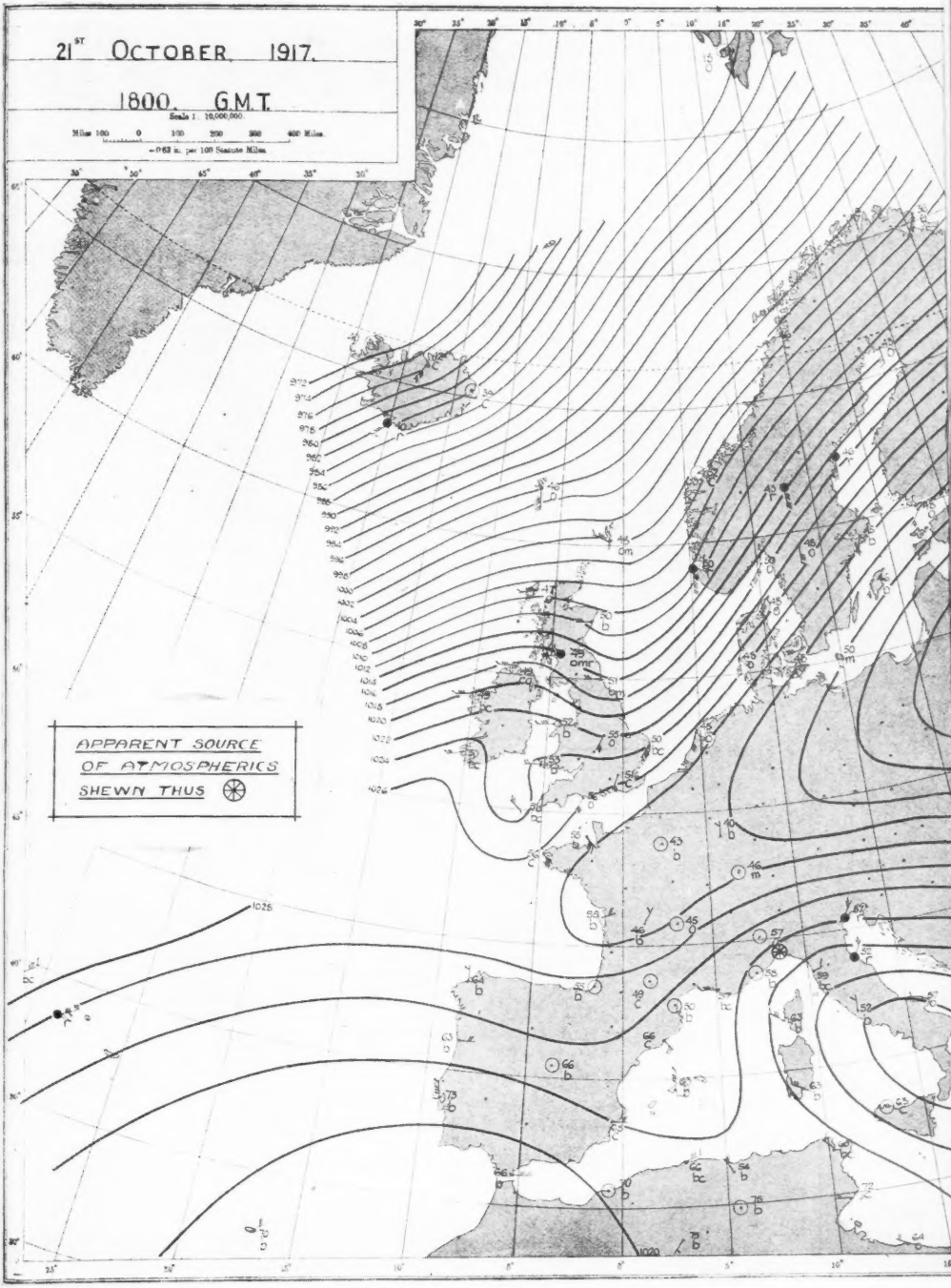
The data were not sufficient to enable us to decide the extent to which the magnitude of the meteorological disturbance could be inferred from these observations, but later instruments and methods are such that I believe an *ad hoc* network would give data of considerable precision as to the relative gravity of the warning.

I have used the British Isles as a test region because the meteorological data available for checking results is fairly complete. But it is not in relation to flying over the British Isles that I regard the results as of first importance. The very conditions which enable us to check results may be regarded as reducing the necessity for the radiotelegraphic direction-finding network. More important, however, is the fact that the British thunderstorm is a poor subject for work of this kind. The discharges in the average British storm are so infrequent that direction-finding on such a source, by aural methods, is difficult in the extreme.

Conditions are, however, very different in the sub-tropical and tropical regions which must be traversed by some of the most important airship routes of the future. Here the meteorological network is so open that its mesh must be reduced by new stations serving the route. I believe that provision for directional observations on atmospherics will considerably reduce the number of stations required for the service of a given route. The storms of these regions have long-sustained and frequent discharges which make them admirable subjects for direction-finding. I have myself, in a very limited experience of such regions, seen several storms in which a flash frequency of one per second was sustained for several hours, and I am informed that these were very trifling off-season disturbances. I have also seen flashes which appeared to be absolutely continuous for five seconds, ordinary direction-finders would in such a case locate the centre of each individual flash.

I should like to refer to a fascinating class of locations which may, or may not, have been false alarms from the pilot's point of view. On October 21st, 1917, at 6 p.m., three British stations indicated a source of atmospherics at 44° 30′ N., 8° 20′ E., near Turin. But the official meteorological records in Italy showed good weather at and around Turin. Ten days later five stations indicated a location at 43° 30′ N., 9° 0′ E., north of Corsica, again in a region of good weather. Yet as Figs. 1a and 1b show, the locations fell on the troughs of depressions. It may be suggested that these, and others of similar character, were bad locations, the actual source of atmospherics lying in the adjacent rain areas. But it seems very difficult to believe that the waves radiated from the nearest reported rain area could have been so cunningly deviated as to give a spurious location. In the first case, for example, the changes in direction to give an apparent source at the point indicated, when the true source was at the nearest station reporting rain, would have to be graded from 5½° to 10° for the stations observing, while in the second case five carefully selected errors between 7° and 13° in the opposite direction would be required.

[The problem of the effect on thunderstorm location of the errors observed in direction-finding on signals is, as Sir Napier Shaw has indicated, one that has to be faced in any proposals for a system such as I have outlined. The accuracy of a single observed bearing of atmospherics was, in the work whose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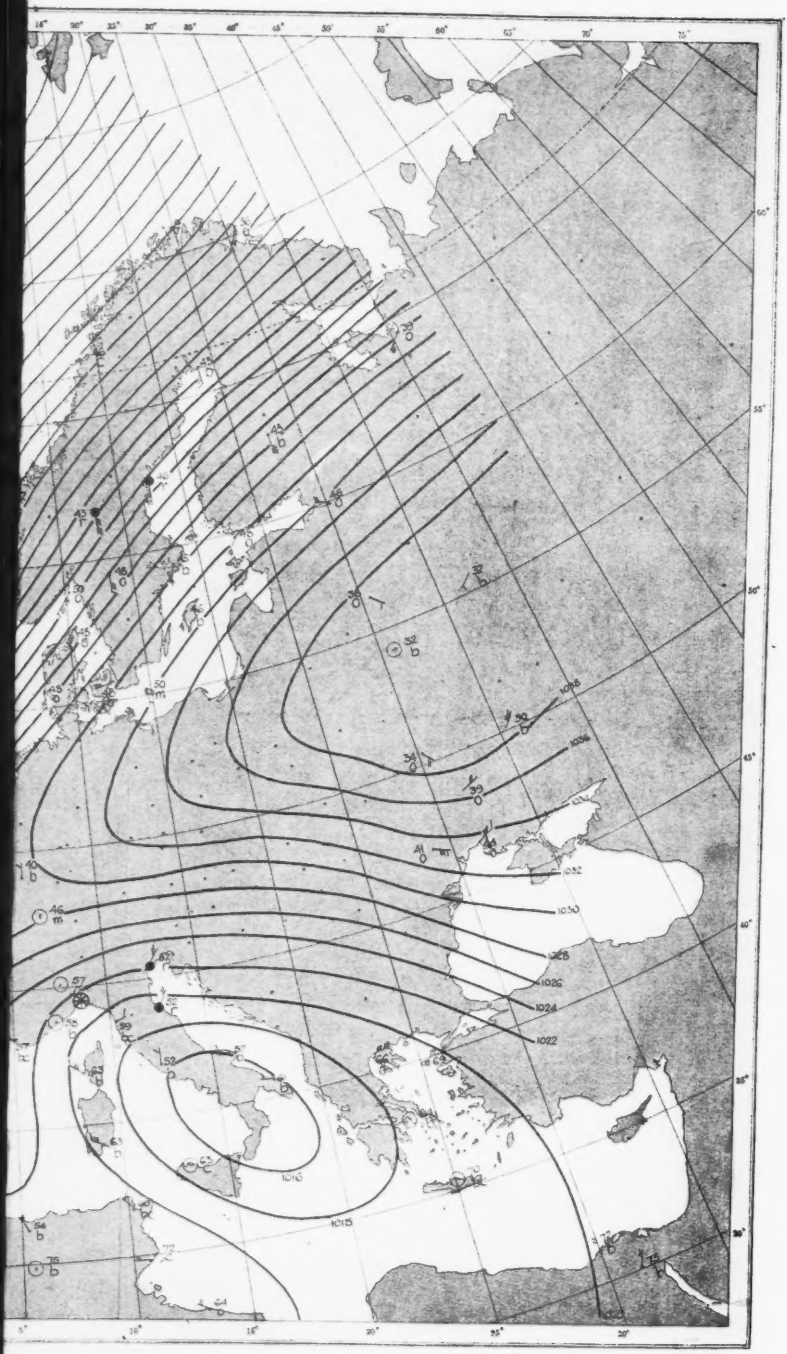
APPARENT SOURCE OF ATMOSPHERICS SHEWN THUS

FIG. 1A.



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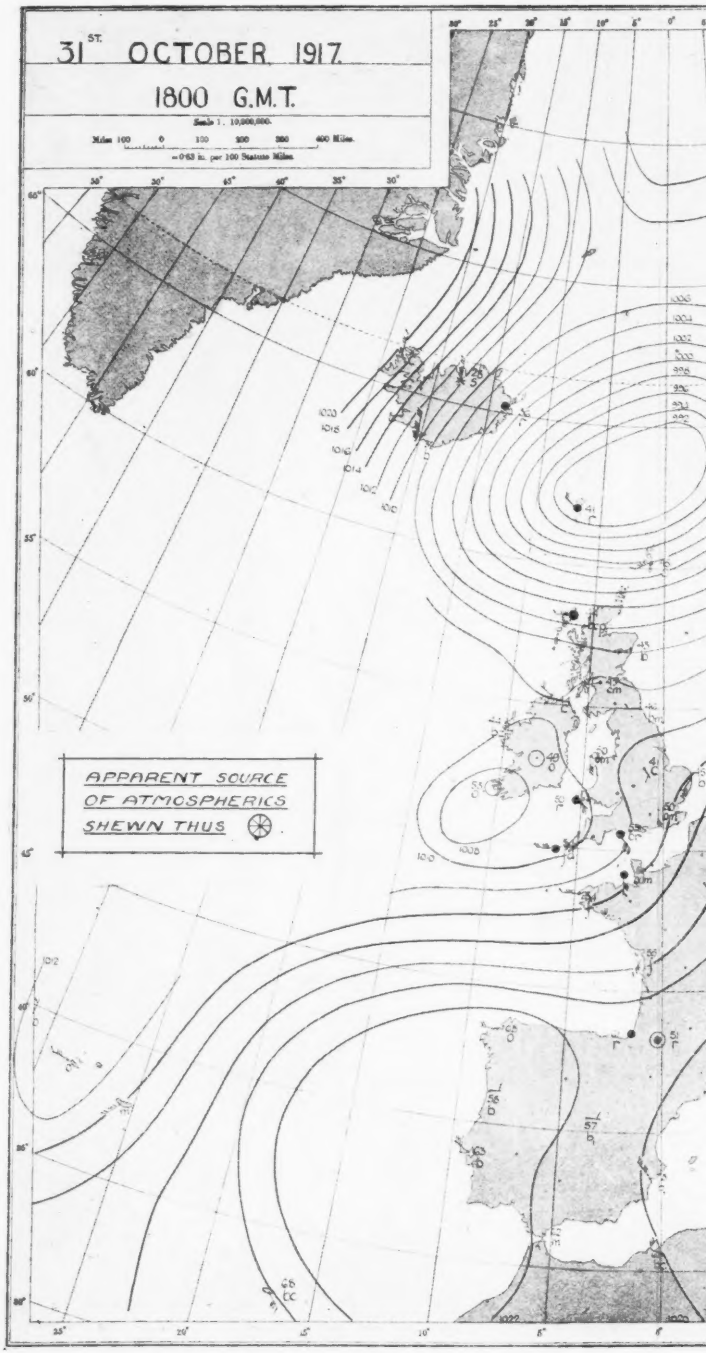
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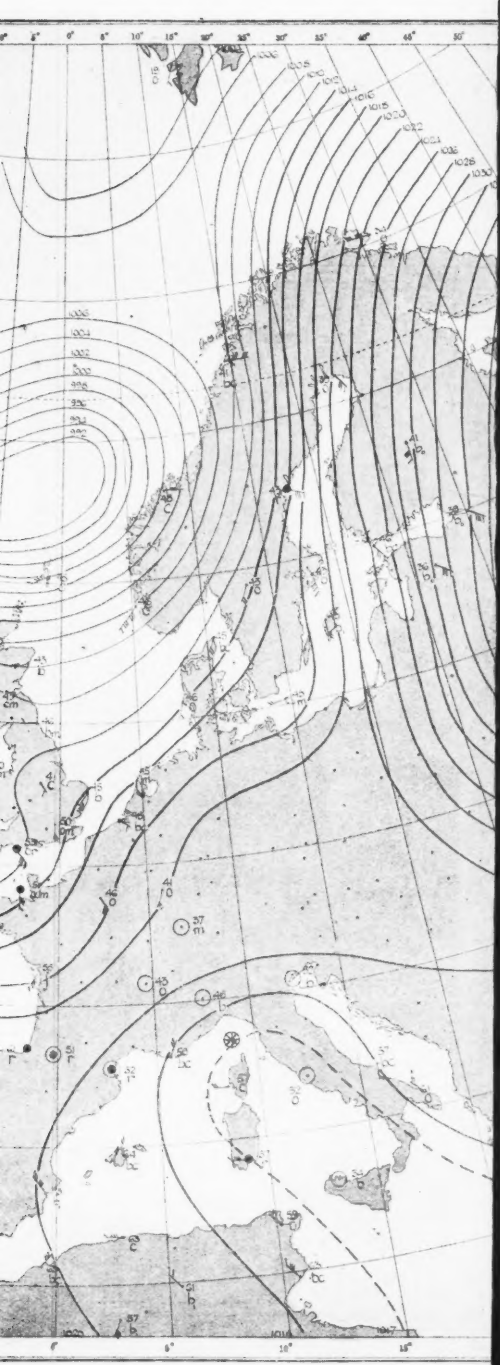

FIG. 1B.

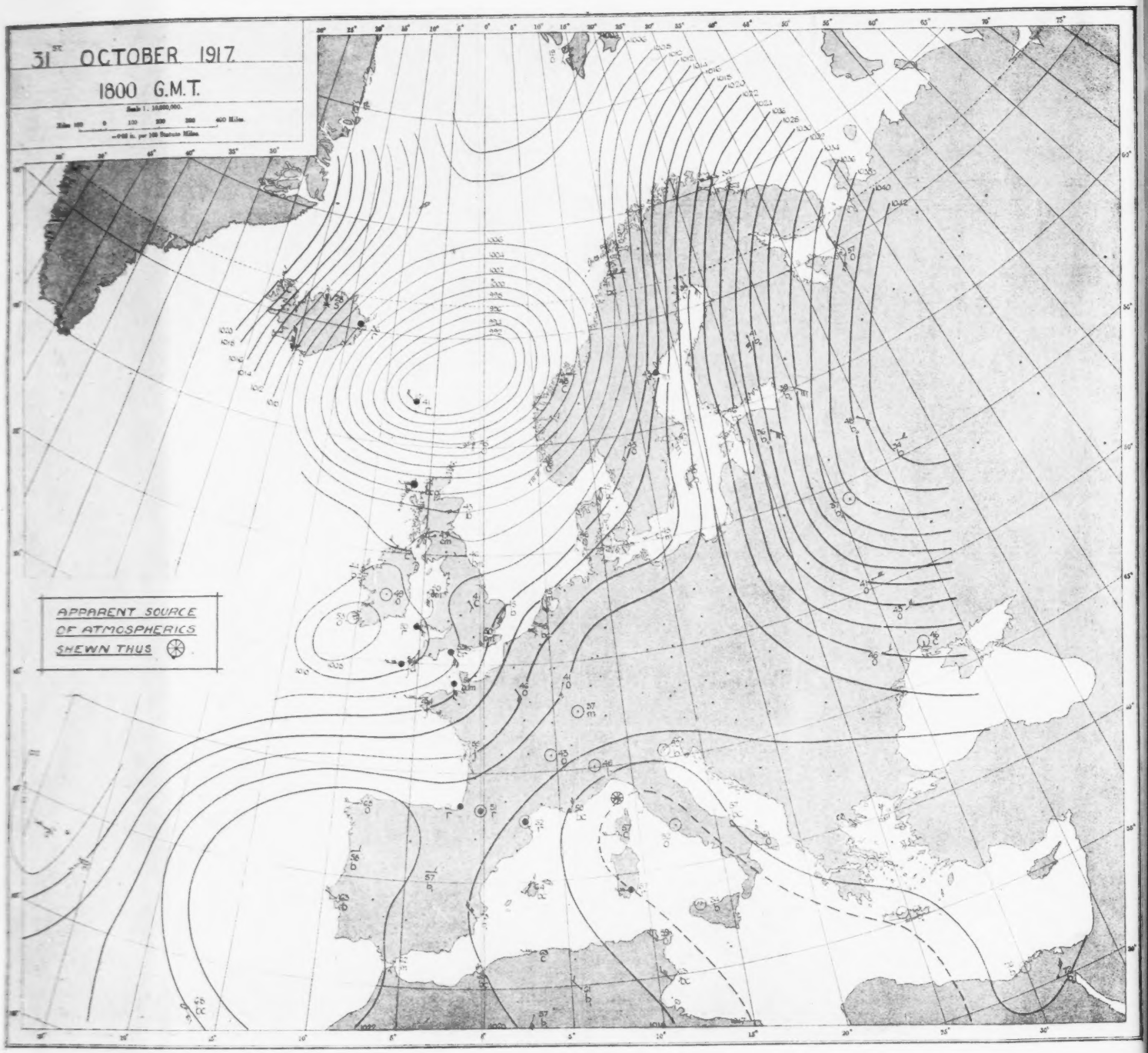
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FIG. 1B.

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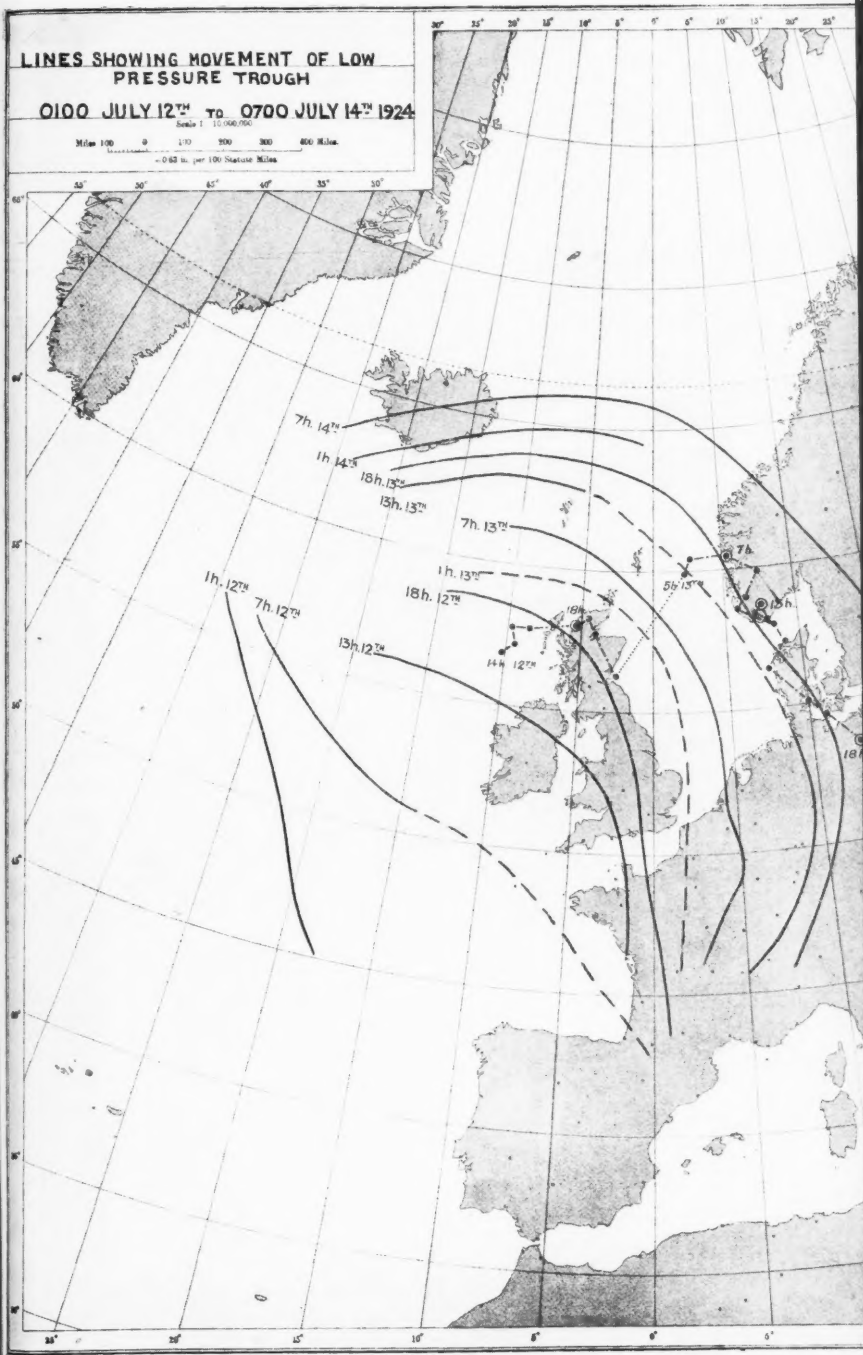
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0100 JULY 12TH TO 0700 JULY 14TH 1924
Miles 100 0 100 200 300 400 Miles.
0·63 in. per 100 Statute Miles
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1h. 14TH
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13h. 12TH
14h. 12TH
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15h.

FIG. 2.

G. 2.

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results I have cited, assumed to be within 5°, and adjustments were made in plotting the observations, when the unadjusted bearings failed to give a good location. Of the 1,600 bearings participating in these 500 locations, however, only two per cent. were adjusted by as much as 5°, five per cent. by 3° or 4°, and six per cent. by 1° or 2°. The most frequently required adjustment was $2\frac{1}{4}$°; this gives a measure of the accuracy with which these difficult determinations were made by the personnel of the coast stations concerned. In 65 per cent. of the locations three unadjusted bearings met in a point. In one of the two Italian cases cited no adjustment was made to any of the three bearings involved; in the other case three unadjusted bearings gave a location which was cut by two more bearings after these had been adjusted by 3°.

Now the evidence from observations on known transmitting stations, although still difficult to interpret, shows clearly that during daylight hours the relative number of occasions when the instantaneous observed bearing differs from the annual mean observed bearing, *i.e.*, when the "variable" error differs from the "permanent" error, by more than 2° is of the order of three per cent., while differences of 5° or over form a small fraction of one per cent. of all observations.* In the hours of darkness these percentages rise to the order of thirty per cent. and seven per cent. respectively. There is no clear indication of a relationship between error and wave-length or between error and distance of source, but it may be inferred from the published data that the error certainly does not increase rapidly with wave-length or distance. There is also some evidence that the errors of bearing are more dependent on something peculiar to the transmitting station than on wave-length or on distance.

Were there a measurable deviation we might expect to find a dispersion in apparent azimuth amongst the component frequencies of the atmospheric, and a consequent change of apparent azimuth of arrival with the wave-length to which the direction-finder is tuned. This is very rarely experienced; in fact, one of the stations taking part in the observations discussed operated on a wave-length ten to fifteen times that of the other stations; despite this fact, there was no marked difference in the frequency with which it and the other stations participated in verified locations.

It seems very improbable, then, that our hypothetical network would obtain erroneous locations; firstly, because of the improbability of occurrence of sensible errors in individual bearings; secondly, because such errors, should they occur, are most unlikely to result in apparent bearings of such special values as to converge in a false location; the worst that is likely to happen is that a location may be more or less completely spoilt, without being replaced by a misleading location. This is *a fortiori* true of daytime observations, in which the navigator is particularly interested. In dark hours he can rely on his eyes as lightning detectors with a range of some hundreds of kilometres. But the proof of the pudding is always a matter of experiment, and the main evidence against the presence of notable errors in bearing, even at night, lies in the results themselves, as summarised in Table I., and particularly in the detailed comparison with reported thunderstorms which showed, as already mentioned, that in ten per cent. of all the cases examined meteorological records showed a thunderstorm within fifty kilometres of the apparent source of atmospherics. I rely also, in this matter of error of bearing, on the vanishingly small probability that, in the two cases of Figs. 1A and 1B (which are not alone in the series), the very accurate coincidence between the apparent source of atmospherics and the trough line of the depression could have been produced by errors which would, as briefly indicated above, have had values graded from $+5\frac{1}{2}$° at Lizard and +10° at Peterhead in the first case to −7° at Lowestoft and −13° at Prawle in the

* Smith-Rose, "Variations of Apparent Bearings," Radio Research Board, Special Report No. 2, H.M.S.O., Tables 24-27.

second case. I feel that this coincidence, at the sunset period when errors of bearing are at their worst, excludes almost completely the possibility of such errors being effective in any notable percentage of directional observations on atmospherics.]

Having thus confessed to apparent failures which may eventually be more instructive than our little successes, I may be allowed to refer to some of the cases which pleased us.

At 5.30 a.m., on November 1st, 1917, three British stations gave a good intersection off the north coast of Sicily, near Trapani. (I call an intersection good when the bearings come within 10 miles of meeting in a point.) Trapani reported lightning to northward during the night hours. Trapani is 1,500 miles from Peterhead, the most distant station participating in the location.

At 8 p.m. of September 20th, 1916, four British stations gave an intersection in the Bay of Naples. Thunderstorms were occurring in Naples and all the surrounding country at this time. Naples is about 1,200 miles from the most distant participating station.

At 1.30 p.m., on July 29th, 1917, six British stations reported bearings all meeting at the town of Helmond, near the eastern frontier of Holland. The Dutch Meteorological Service informs us that thunder began at Helmond at 1.5 p.m., and the thunderstorm continued till after 2 p.m. Helmond is 165 miles from the nearest and 600 miles from the most distant station participating.

A storm, N.E. of Paris, was located with an accuracy of a few kilometres on May 10th, 1917; a storm at Orleans was similarly located on July 4th, 1917, and so on.

The greatest distance at which a location of an apparent source of atmospherics was confirmed by meteorological data, other than a report of lightning, in this series of observations, was in the case of a location in Greece on November 14th, 1916. The apparent source, at 39° 30′ N., 21° 30′ E., was on the centre of a depression. The distance between this point and the most distant participating station is 1,500 miles. In another instance a location fell in the trough of a depression, at Durazzo, Albania, on October 11th, 1917. The distance in this case was 1,450 miles. Two hours later a new location, from observations in which one of the three participating stations was replaced by another station, was found at Lissa, Dalmatia.

Lest it be thought that the method is only applicable at great distances, it may be noted that the azimuth of lightning five miles distant has been determined by direction-finding, and that observations at Aldershot on June 18th, 1918, discriminated between West and East London in the case of a thunderstorm which displaced much wood paving in North Kensington.

Very recently some interesting results have been obtained by the replacement of aural methods by a recording system. The directional recorders used were designed and constructed at the Radio Research Board's station at Aldershot (now transferred to Ditton Park, near Slough). The instrument consists of a frame aerial about two metres square, which is rotated continuously about a vertical axis at a speed of four revolutions per hour by means of a small turret clock. The main axle of the frame carries a fork which imparts rotation to a recording drum. The drum is attached to a lead-screw which feeds down through a fixed nut, so that the drum rotates at the same speed as the frame, but descends by three millimetres during each revolution. The electromotive force developed across the condenser which tunes the frame aerial to a wave-length of 20,000 metres is magnified by a multi-stage amplifier, and the resulting voltage released produces a vertical deflection of a silver syphon pen which writes on the chart carried by the recording drum. It will be seen that in the absence of

atmospherics this pen inscribes a helical trace on the chart, but that the arrival of an atmospheric is marked by a short line transverse to this helical base line, the amplitude of excursion being to some extent a measure of the electromotive force produced in the frame by the atmospheric, and its position indicating the time of arrival of the atmospheric and also the instantaneous azimuth of the plane of the frame aerial at that moment. The chart, 49 cm. by 34 cm., carries the record for twenty-four hours, and from it can be extracted the mean apparent direction of arrival of atmospherics and the mean intensity of disturbance for each hour of the day. With this instrument it has been found possible to watch the approach of a thunderstorm for three or four hours before its arrival at the recording station, and it is exceedingly interesting to sit, with the recording drum beside one's desk, and watch the development of the disturbance and the slow displacement of the region of densest markings on the chart as the azimuth of the approaching storm alters. Mere visual inspection of the chart *in situ* is usually sufficient for the determination within 10° of the azimuth of greatest disturbance, greater accuracy can of course be attained by measurement. One recorder has been in operation at Aldershot for some years, a second began work at Lerwick at 10 a.m. on July 12th, 1924. At 2 p.m. on that day the directions of arrival of atmospherics at the two stations indicated a source off the Hebrides. This source was followed across Scotland, being over Ross-shire at 6 p.m. and May Island at 9 p.m. These locations coincide with reported thunderstorms. For six dark hours neither record could be interpreted directionally, but at 6 a.m. on the 13th they placed the source halfway across the North Sea. After spending the morning over Norway, the source reached Stavanger at 1 p.m., a thunderstorm was occurring at that time and place. The source was followed across Denmark and Germany, at 6 p.m. it was near Neu Strelitz. At 10 p.m. it was 120 miles south of Posen, at 1 a.m. it was apparently 500 miles south-east of Posen; a thunderstorm was in progress at Posen at the latter hour. By this time the difference in bearing at the two stations was 20°, its continued decrease leaves the accuracy of later intersections in doubt. At 3 a.m. on the 14th the source appeared to be on the Roumanian coast of the Black Sea.

The general meteorological situation during this period was marked by the advance across Europe of a well-marked trough of low pressure associated with a cold front. The progress of this trough is shown by the isochrones of Fig. 2, which also shows the trajectory of the apparent source of atmospherics, which will be seen to be, in general, a little in advance of the actual pressure minimum.

On other occasions during the same month, the first month of their simultaneous running, the two recorders were successful in locating thunderstorms in Baden on the 16th, near Berlin on the 18th, at Oxford on the 21st, at Dortmund on the 22nd, and Emden on the 24th, confirmation from local observation being available in all these cases.

[The recorders at Ditton Park and Lerwick form the first links of a miniature "Empire Chain" of atmospheric recorders which are now coming into operation. A third recorder has just begun work in Egypt, a fourth is being set up in Southern India, and it is expected that others will soon be in operation in South Africa and in Bengal. It is to be hoped that other recorders may be set up in Europe to bridge the somewhat wide gap between the British Isles and Egypt.

It is of interest to consider the possible sites for a group of recorders to make a preliminary test of a thunderstorm locating chain for the airship route to India. It is perhaps unfortunate that a first requirement of such a chain is that its stations should not lie on the route itself, since this would give very unfavourable trigonometric conditions for the location of storms actually on the route. This difficulty is, however, minimised by the long range of the apparatus, enabling us to take advantage of existing service bases for the accommodation of the

recorders. If we assign to the standard pattern recorder an effective range, for location purposes, of about 750 kilometres, then the most economical chain would have recorders at the apices of a chain of linked equilateral triangles of some 1,500 k. side. Political and geographical divisions being incompatible with simple geometry, this ideal chain could not be an "all-red" one, but a satisfactory survey could probably be carried out at little expense by installations at Lerwick, Ditton Park, Gibraltar, Malta, Cyprus, Abu Kir, Khartoum, Baghdad, Aden, Socotra, Karachi, and Bangalore, giving a mean base line of the required order. This scheme would call for the provision of eight recorders in addition to those already existing, the cost of construction and installation would be very moderate, the running costs slight, and the amount of attention required by the instruments is so small that no special personnel are required. A year's running of such a simple system would, I believe, be very valuable in filling gaps in our knowledge of the electrical conditions on the route.

The question of the modification of the system to meet the need for rapid communication of observations to the aircraft using the route might well be left for discussion while the recorders make the preliminary survey.

It may be added that such a survey would probably be of considerable value to the radiotelegraphist, who is at present unable to exclude the effects of atmospherics from his receiving gear, and is therefore compelled to rely on the choice of quiet sites and directional receivers to minimise the interference caused by atmospherics. The selection of new sites and of the most suitable lines of communication could be undertaken with far greater confidence were the results of such a survey as is here outlined available as a guide.]

Reverting to the basis of the suggested warning system then, I think it may be asserted that condition (*e*) can be satisfied, in that there are, in the data from stations which did not specialise in the work, few cases in which meteorological disturbances calling for care in navigation were not found in the region which would have been covered by a "source of atmospherics" warning; condition (*d*) appears to be almost too completely satisfied in the cases where isobaric irregularities or rain alone were found; condition (*c*) is certainly fulfilled in respect of space limits, warnings of disturbance 1,000 to 1,500 miles distant would be embarrassingly "grandmotherly." In respect of time, everything depends on the organisation of the network, but the only case in which the system must necessarily break down is that in which a storm suddenly breaks out just at or just ahead of the instantaneous position of the aircraft. This must be a relatively rare case, but in view of the disturbance of electric field produced by the craft itself, especially if it be an airship, the risk of trigging off the first discharge in an unstable region must arise. Much turns on whether a storm starts off with a big discharge or whether we may rely on the occurrence of minor discharges as premonitory signals. Our data do not suffice to decide this point. I fear we have many more cases in which sources were located at places where thunderstorms occurred several hours earlier than conversely; one of our earliest locations, for example, was in Poole Harbour, on April 28th, 1916, at 9 p.m. A thunderstorm had occurred there at 4.30 p.m.

Condition (*b*) is fulfilled in respect of accuracy; when a location is obtained it is, I think, usually accurate enough for navigation clear of the disturbance, but again inadequate data leave us in doubt whether a guarantee could be given that the location of the most prominent source in respect of the propagation of atmospherics would coincide with the source nearest to the air route or most dangerous to aircraft. Experiments on a tropical route are required to decide the probabilities. I may, however, say that the comparatively crude methods which I have discussed do not represent the limits of immediate practicability.

As I have already indicated, discussion of condition (*a*) would carry me far beyond the limits of time and space allotted to me. Here again I will only say

that if specialist observers on an air route cannot with the standard direction-finding equipment distinguish the particular atmospherics proceeding from near lightning, then new equipment can be produced to enable them to do so.

[It should be noted, however, that there are meteorological conditions in which no aircraft is likely to survive, which are yet unaccompanied, so far as has been determined, by the radiation of atmospherics as warning signals. Thus, Father Gherzi, S.J.,* finds no relation between the direction of arrival of atmospherics at Shanghai and the azimuth of typhoons.]

Turning to another aspect of the study of atmospherics, still with particular reference to the means by which we may turn atmospherics to good account, I should like to refer briefly to the methods by which Professor Appleton, Mr. J. F. Herd and I have been investigating the characteristics of individual atmospherics with the multiple purposes of establishing their origin, explaining their interferent properties and extracting from them information as to the mechanism of thunderstorms.

The method used is to select a simple aerial circuit whose constants are such that the electro-motive force produced across two given points by an atmospheric is linearly related to the change of field strength in space constituting the atmospheric. This electro-motive force is magnified by a triode amplifier which is arranged to be free from measurable distortion, and it is then examined by means of a cathode ray oscillograph.

In this instrument a voltage applied between two deflecting plates moves an electron beam in such a way that the displacement of the bright spot produced by the beam impinging on a fluorescent screen is directly proportional to the applied voltage. A second pair of plates, at right angles to the first pair, are supplied with a locally generated voltage which deflects the spot in such a way that it travels uniformly from left to right in a selected time which may vary from a five thousandth of a second upwards, and then shoots back to its initial position in a very small fraction of this selected time. Under the joint influences of the magnified voltage from the atmospheric and of this timing voltage the spot describes a curve which is a graph of the temporal variation of field strength in the atmospheric, described on a linear time base. This curve is copied by hand, and the maximum change of field, the total duration of the disturbance and other characteristics are measured from the drawing. Persistence of fluorescence and persistence of vision enable us thus to deal with details of the atmospheric lasting for only one forty thousandth of a second and to obtain a clear picture of their shape. A series of slides, not reproduced, show the shapes of typical atmospherics and their fine structure.

It is clear from the specimens shown that although the main field disturbance is a comparatively harmless looking one of long duration, about two thousandths of a second in the case of the oscillatory disturbances and just over a thousandth of a second in the sensibly aperiodic disturbance, yet these fundamental forms carry an embroidery of fine detail which is really responsible for the noise produced in the radio receiver. This embroidery may be so fine as to go beyond the discriminating power of our present apparatus, but the slides show examples in which an element of the disturbance, having an amplitude very much greater than that of a signal wave, has a duration comparable with one or two periods only of such a wave and a shape so far from sinusoidal that its short period components in turn are comparable with or greater in amplitude than short signal waves. Until the very great mass of material now being examined, consisting of over 50,000 samples of atmospherics from England and the Middle East, have been dealt with it would be unsatisfactory to enlarge on these matters beyond

* "Recherches sur la Marche des Typhons," l'Onde Electrique, 3, No. 34, p. 491, 1924.

the remark that there is emphatically no mystery about the reasons why atmospherics are so troublesome!

[It is by methods such as these that I believe we shall be able to supplement directional observations in such a way as to provide a specification of the nature of the meteorological disturbance located, and I think it is in nowise fantastic to visualise a group of twelve radiotelegraphic observatories capable of keeping effective watch on the thunderstorm conditions of the three continents of the "Old World."]

To sum up, I have tried to show you that the atmospheric, mischievous as he is, may be made a useful slave, that he is probably less fallible as a warning agent against the thunderstorm and its dangers than are we poor human meteorologists, and that by having him sit for his portrait we may extract from him information as to the mechanism of the lightning discharge; in short, that he is by no means devoid of beneficence.

I am indebted to the Meteorological Office for facilities for the early work described in this lecture and for recent data, and to the Radio Research Board established under the Department of Scientific and Industrial Research for the provision of facilities for the more recent work and for permission to publish the results. It may, however, be well to add that the opinions expressed are purely personal views.

Discussion

Admiral of the Fleet Sir Henry Jackson, in opening the discussion, said that probably most people before reading Mr. Watson Watt's paper looked upon thunder and lightning as nuisances, but he believed that life might be worse without them. The lecturer had certainly already found a certain amount of use in the atmospherics as being guides for indicating the presence of lightning. There was still a great deal to be found out, and the author was undoubtedly the best man to do this. He had been viewed with some suspicion about ten years ago when he prophesied the results he would be able to obtain, but everything he foretold had come true.

Experiments on a larger scale were now being arranged for at Slough so that he could continue his investigations.

There were two points in the lecture which seemed to need more explanation. The lecturer had stated in his paper that all the observing stations concerned were situated in the British Isles, yet in the table a certain percentage of thunderstorms were quoted as having been located in Spain at a distance of 250 kilometres. Now Spain was a good deal farther away than 250 kilometres. Perhaps the lecturer would explain what was meant.

The other point was about the disturbance which was located at Turin. It was at six o'clock in the evening, when it was dark. This position might be due to "night effect" distorting the bearings. Atmospherics should be as liable to deflections in bearings, caused by Nature, as signals.

Experiments in aircraft to ascertain the bearing of thunderstorms are very desirable, so as to aid them in avoiding those localities.

Sir Napier Shaw joined his congratulations to Sir Henry Jackson's on the lecturer's paper, which he had found particularly interesting as he had been associated with the commencement of the work. He did not know then that a connection had already been traced by Sir Henry Jackson between thunderstorms and atmospherics. He was glad to have a reference to the account, because these historical points were of interest to those who were keenly concerned in the development of new ideas on certain subjects.

He could recollect one stage of the present investigation which was clouded over by someone—he forgot who it was—who objected that refraction was so great that no conclusion could possibly be drawn from the intersection of gnomonic lines on the map. It was asking for misleading information rather than a process indicative of where a storm was! It came to him as an agreeable surprise that there was now no difficulty about lines on the map; they were simply drawn straight across. Distant lightning flashes were actually located thereby within 10 kilometres of the observation. The one or two cases where notable differences are apparent, might possibly be accounted for in alteration of direction. That seemed to him to be the more likely cause in the Turin case, because of the peculiar geographical position of Turin, relative to this country. The line of Alps running up to 15,000 feet in height lay between it and the observing stations, and might well cause some deviation.

It was interesting to track down different sources of atmospherics, and particularly the sources in progressive lines of trough, advancing across the country. The line of trough is notably a line of very marked penetrative convection—of strong up-currents, and these penetrative convections, which used to be regarded as the most ordinary form of meteorological experience, are now regarded as things which it is rather difficult to arrange for. He meant that one had to deal with meteorological conditions rather skilfully in order to get one of these upward currents of air which might give rise to thunderstorms.

Captain Douglas, when photographing layers of cloud from above, found a considerable number of notable projections from the horizontal cloud sheet. Shooting up from the cloud sheet were little projections which produced rain. They did not last very long. They were just the sort of thing that might give rise to something comparable with a lightning discharge, and these phenomena were most likely to occur in the line of trough which was indicated as a source of atmospherics.

He wondered if aviators had any evidence of electrical discharges in circumstances of that kind. They might possibly be visible occasionally to someone in an aeroplane. Perhaps, however, the flash might only be visible at night when no one was likely to go up to look at the other side of a big layer of cloud.

With regard to the subject's apparent lack of appeal to members of the Society, he had noticed in the " XIXth Century " of that month, that someone had drawn attention to the force of the machine habit in modern life. People were so accustomed to using machines, making machines, working with machines, under machines, and on, that they naturally, on seeing a lecture programme, for example, looked for a subject where a machine came in; and when the machine did not appear to come in they waited until someone introduced one.

He added that the inquiry that the lecturer had been conducting was not yet in the stage in which the machine could come in. He hoped the lecturer would not be disturbed by the machine habit and would continue his work until they were on safe and sure ground on this most interesting subject.

Mr. C. J. P. Cave said he was much interested in the results about which the lecturer had been telling them, as he was at Farnborough when the work started.

In 1915, Mr. Watson Watt suggested the cathode ray tube, but there was then great difficulty in getting a cathode ray tube, and one was not obtained until after he had left Farnborough, and it was not until some years after that he saw it at work. He was sure that the audience would be delighted when they saw the demonstration with it. The whole of the work was extremely skilful.

Squadron Leader N. H. Hamilton asked the lecturer if he had tried grouping the various types of atmospherics according to their wave lengths, and,

if so, did one type of atmospheric appear with one particular wave-length. Or, in other words, was an atmospheric in any way consistent?

Mr. J. F. HERD said that when the lecturer told him the title of the paper he had doubted its wisdom, but on reading the manuscript he agreed that Mr. Watson Watt had amply established his point, and he had no doubt that after hearing the paper those present would also agree. The importance of the subject to aviators, in the matter of locating thunderstorms, was very great indeed. The ordinary meteorological network might suffice for such a small area as the British Isles, but it was more than possible that the use of direction-finding of atmospherics might prove of great value in the location of thunderstorms occurring or forming outside the region covered by such a network. All Europe, and possibly a much larger area than Europe could be dealt with by four or five co-ordinated stations.

The arrangement of the cathode ray oscillograph for the examination of the wave-form of atmospherics—as described in the lecture—also gave a very useful indication of the existence of thunderstorms, sometimes at places or at distances outside the ordinary meteorological network. In particular he might quote one case which occurred on a certain Sunday last summer. Observing on the oscillograph about ten o'clock in the morning, he concluded from the indications that thunder was occurring at no great distance, and might be expected in England. In the afternoon it came, and the latter part of the day was characterised by very great thunderstorm activity over all the South, South East and East of England. It was subsequently established that, at the time of the morning observation referred to, a thunderstorm was occurring well off Cornwall, and therefore outside the British meteorological purview, although it developed over England later. This afforded an excellent and by no means isolated example of the possibilities of thunderstorm warning, and other cases might be quoted when, from such observations, he would have deduced the existence of thunderstorms at no very great distances, these conclusions being subsequently confirmed by definite reports of thunderstorms. The use of the cathode ray tube in this manner, in conjunction with direction-finding (either by the methods described in the paper or by improved methods) might well prove of considerable benefit to the aviator.

Colonel TIZARD, before calling upon the lecturer to reply, said that though he had been in touch with the investigations for some years he had not realised how close the correlation was before.

Perhaps the lecturer could give them some indication of where stations might be established in order to be able to explore for thunderstorms along an air route to India? This might become a practical question in the near future.

Mr. WATSON WATT, in reply, apologised for the ambiguity caused by the condensation of data in the table to which Sir Henry Jackson had referred. The distance limits of 250 kilometres and 250-1,000 kilometres in the table were not distances from the direction-finding stations to the sources of atmospherics, but distances between the apparent source of atmospherics and the nearest thunderstorm reported by a meteorological observer.

[A fuller treatment of the question of errors of bearing, and of other matters arising out of the discussion will be found added, in square brackets, at various points in the main text.]

He could at once give Sir Napier Shaw the reference for which he had asked. He would find it in the Proceedings of the Royal Society for May 1902.

With regard to Sir Napier Shaw's remarks as to the interposition of land prominences, of four or five kilometres height, between apparent source and observer in the two cases of Figs. 1A and 1B he would regard these prominences as very minute surface irregularities in relation to the wave-lengths of a thousand kilometres involved, and would expect that the disturbance produced by such

minor irregularities would be negligible outside a region comparable with the dimensions of the irregularity. In support of this view, the following accurate locations may be cited :—

(1) 8 p.m. September 30th, 1916, near Turin. Thunderstorm reported 40k. away two hours later.

(2) 7 p.m. November 21st, 1916, near Apice. Thunderstorm reported 55k. away three hours later.

(3) 7.30 p.m. August 26th, 1917, W. from Turin. Thunderstorm reported 30k. away two hours later.

In these cases of propagation along an almost identical route, then, the presence of the Alps did not produce errors reaching $2\frac{1}{2}°$. The successful locations at Trapani and Naples should also be noted.

Like Sir Napier Shaw, he had hoped for observations from aircraft of minor lightning discharges above cloud. He had, in fact, hoped that the Chairman, with his talent for being in the right place at the right time, might have been flying over Turin on the evening in question.

In reply to Major Hamilton, he said that the fundamental wave-lengths of atmospherics fell quite outside the commercial range, they lay between half a million and a million metres, but the embroidery on these fundamental forms might be comparable to wave-lengths of a few thousand metres. Moreover the form and incidence of these short period components is such that the amplitude of the components corresponding to wave-lengths of a few hundred metres is still comparable with signal amplitudes. Atmospherics are, however, quite consistent, and the work of classification and analysis is going satisfactorly.

With reference to the Chairman's remarks, he was very keenly interested in the possibility of placing stations abroad.

[A paragraph has been added to the paper dealing with this question.]

After the lecture a demonstration was given of the use of the cathode ray oscillograph for the delineation of wave-forms of atmospherics. An artificial aerial was used, with a generator of artificial atmospherics; by suitable adjustment of the electrical constants of the generator many typical forms of atmospherics could be simulated. Thanks are due to Mr. J. F. Herd, A.M.I.E.E., who prepared and gave the demonstration, to Messrs. M. L. Magneto Syndicate, Ltd., for the loan of the sources of high voltage, and to Messrs. Chloride Storage Battery Co., Ltd., for the loan of sources of low voltage used in the demonstration.

Colonel TIZARD said that he was sure the audience were very grateful to the lecturer and his collaborators for the trouble they had taken in arranging this most interesting demonstration, and a hearty vote of thanks was passed.

CAYLEY ON AIRSHIPS.

Sir George Cayley (1773-1857) has been described as the "Father of British Aeronautics" on account of the extraordinary foresight he showed in appreciating the salient features necessary for the success of both airships and aeroplanes. His articles on the latter appeared in "Nicholson's Journal" during 1809 and 1810 and were reprinted by the Aeronautical Society in "Aeronautical Classic, No. 1," but his airship work, although of equal importance, has not received the same recognition.

The first papers he wrote on the lighter-than-air aspect of the subject were published in Tilloch's "Philosophical Magazine" in 1816 and 1817. Since these articles have not previously been reprinted and the volumes containing them are now rare, they are reproduced here from copies which have lately come into my possession. Cayley contributed later articles on airships to the "Mechanics' Magazine" in 1837, which were reprinted in "Aeronautics," Vol. II. (1909), p. 142, and Vol. III. (1910), p. 1.

W. L. M.

I.

TILLOCH'S PHILOSOPHICAL MAGAZINE, Vol. XLVII., No. 214, Feb., 1816, p. 81.

TO MR. TILLOCH.

SIR,—I am glad to find that the public attention is called to aërial navigation by Mr. Evans, in your Magazine for November last. This subject is of great importance to mankind, and is worthy of more attention than is bestowed upon it. An uninterrupted navigable ocean, that comes to the threshold of every man's door, ought not to be neglected as a source of human gratification and advantage. Mr. Evans proposes the action of a large inclined plane suspended below a common balloon, as the means of making it take an oblique course in its ascent, and by means of the same plane to make the weight of the apparatus cause an oblique descent towards the same point of steerage. This principle is unquestionably capable of performing what that gentleman proposes, although the construction he has given is only adapted to effect the purpose in ascending, as the want of weight in the plane would prevent it from operating, excepting in a very limited degree, in the descent. In the small balloons used by Mr. Evans, the plane was, in fact, the whole burthen supported; and hence, under the small velocity generated, the descent was as oblique as the ascent. But in the balloon 80 feet in diameter described by that gentleman, the plane in ascending would receive, according to his estimate, above 2,000 pounds of resistance from the air, and thus become efficient; whereas in descending it could only sustain a resistance equal to its weight, which of course will be as little as possible, and hence it will be nearly inefficient. The general principle, however, is perfectly true; and when applied advantageously, although it is an indirect way of gaining the proposed horizontal point, yet it will be as effectual as the process of tacking in ordinary navigation. Mr. Evans estimates that a Montgolfier balloon of 80 feet in diameter, with a plane suspended under it in an angle of 70° with a perpendicular line, the dimensions of which are as 1.4 to 1, compared with the great circle of the balloon, will be carried through the air by a power of ascent equal to 2,792 pounds with a velocity of 28 feet per second, and hence that the travelling horizontal speed will be about 19 miles per hour.—If the resistance of air be taken at one pound per square foot at a velocity of 23 feet per second, which is two feet more than the common engineering estimate, the resistance of a globe to its great circle, according to Mr. Robins's experiments, as 1 to 2.27, and the resistance of the plane as the square of the sine of the angle of incidence,—

it will require about 12,450 pounds to produce this rate of conveyance in Mr. Evans's balloon; and hence the power he allows is not one-fourth part of what is required. This apparatus would, however, according to my calculation, travel with the speed of about $8\frac{3}{4}$ miles per hour,* which is quite sufficient to show the utility of the principle, whether the mistake be in Mr. Evans's figures or my own.

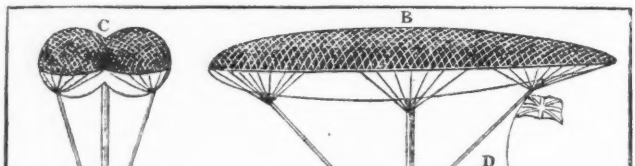
A few years ago I made many experiments upon the power of inclined planes, some of which exceeded 300 square feet in area: an account of these may be seen in Mr. Nicholson's Journal for November 1809, and February and March 1810. It may be affirmed with confidence from these experiments, that in obliquely descending the efficacy and steerage of the inclined plane have been completely ascertained. My object was to leave out the unwieldy bulk of balloons altogether, and to make use of the inclined plane propelled by a light first mover. Although my attention has hitherto been diverted from making further experiments, I am fully convinced that this mode of aerial navigation is practicable, and will, ere long, be accomplished. In the mean time I shall be glad to promote any promising experiments upon the steerage of balloons, and therefore offer the following observations through the medium of your Magazine.

In considering the means for obviating the relative resistance of balloons in passing swiftly through the air, the leading general principle is evidently to increase their dimensions far beyond the limits hitherto adopted. The weight of their superficial materials, and the resistance they meet with, being as the squares of their diameters; whereas their power of support being as the cubes of these diameters, it follows that their power may be made to bear any required proportion to their resistance. Thus a balloon of one yard in diameter meets with ten times more resistance, in proportion to its power, than a balloon of ten yards in diameter.

The next consideration is, that a globe is by no means the best shape for obviating resistance; a greater extension in the line of its path, with a corresponding diminution in the section perpendicular to it, may be adopted with great advantage. Keeping in view these two leading principles, the former of which places the proper scale of experiments beyond the expense that individuals choose to appropriate to such purposes, I propose that the following plan be adopted by those desirous of promoting this noble art:—First, that a subscription be entered into for obtaining a proper fund;—and secondly, that a committee be appointed by the subscribers for the purpose of carrying such experiments as may appear eligible into effect. To such a committee I should be glad to submit the propriety of making the following experiment, which would be capable of trying all the expedients hitherto proposed for steering balloons. The principle proposed by Mr. Evans is only applicable, at present, to fire-balloons; and as these are the cheapest, and upon a large scale with proper precautions may be made safe with respect to fire, I propose that the experiments be made upon the Montgolfier balloon. The scale I propose to adopt, though as small as is compatible with the object in view, will appear to any one who has not calculated the proportions required for the success of the experiment, of stupendous magnitude.—Amazement would have been the consequence of presenting to the imagination of an ancient Briton the idea of a British hundred-gun ship, when only contemplating the principles of navigation exhibited in his humble coracle covered with a skin. From the truck of the flag-staff to the extreme of the bowsprit, a vessel of this sort will measure about 90 yards; and it is a wonderful effort of human ingenuity arising from the gradually accumulating knowledge of ages:—but the *long boat* of aërial navigation commences about the bulk where the man-of-war of common navigation has reached its full growth; and what may

* An error in the paper of Mr. Evans was corrected in our number for December, making the velocity $10\frac{1}{2}$ miles per hour.

be the vessels, *I hope not men of war*, of this sort which a thousand years of human invention may bring to light, I am at a loss to contemplate.



Let A B C represent the plan, side and end elevation of a balloon or aërial vessel, made of woollen cloth, and kept to its shape by light poles attached to it, and internal cross bracings of wire or cord, opposing the tendency to become circular from the internal pressure of the heated air: this vessel to be 15 yards in elevation, 30 in width, and 100 in length. About 27 yards below this vessel must be suspended a convenient boat-shaped car, by six ropes collecting the cordage of the netting. This boat must be furnished with a light fire-grate; and an oval chimney of thin metal must descend from the balloon and cover the fire. This chimney to be furnished with three fine wire nets to prevent sparks from passing up it. A sail or rudder D must be attached to the boat from behind, which can be turned to either side by bracing the boom to which it is fixed. The cloth made use of being woollen will not be subject to take fire; but it is requisite that it should be made air-tight, and likewise impervious to rain, by some coats of paint or varnish on the outside. The machine being thus completed so far as it is necessary to try the principle of the *inclined plane*, as soon as the balloon is inflated, let the front ropes be lengthened and the hinder ones shortened, till it stands in an angle of about 30° with the horizon, when it will be found to rise in an angle of about 45°, and the horizontal velocity towards its destined *harbour* will be about 20 miles per hour.*—The power of the heated air would be about 17,600 pounds: of this about 6,800 pounds would be required to generate the velocity specified, and the remainder will be consumed in the weight of materials, fuel, passengers, &c.

It may seem at the first view extraordinary that I should propose to make use of so long a chimney; and this requires some explanation. The exterior resistance of the air to the anterior portion of the balloon, will amount on some parts of it to about 26 pounds per square yard at the proposed speed; and hence an internal pressure at least equal to this must be created, for the purpose of preserving the form of the balloon. This is most readily effected by force of

* This calculation is grounded upon the following data:—That in Montgolfier balloons one cubic yard of space has been found to give 11 ounces of power—that the form of the vessel will prevent it from receiving more than a third part of the resistance that its greatest cross-section would receive at the same velocity; and that a velocity of twenty-three feet per second in air, creates a resistance of one pound per square foot, according to some careful experiments of my own upon a very large scale.

the long column of heated air passing up such a chimney as I have described; for, were the air within it no hotter than the general temperature of the balloon, the 27 yards of chimney added to half the height of the balloon would create a pressure rather exceeding what is required; and I conceive it will, from its greater rarefaction, create as much pressure as will permit the prow of the balloon to be depressed to an angle of 30° with the horizon, and still enable it to resist the impression of the air in descending. It is scarcely necessary to observe that the balloon must be, as usual, furnished with a large valve at the top, and likewise with one at the bottom, to permit the escape of the hottest or coldest air as required. If the specific heat of air be to that of water as 1.79 to 1, it will require about 880 pounds of fuel to inflate this balloon, exclusive of what will be consumed to supply the waste of heat during the operation; and when a second rise is required, by having suffered the escape of air equal to the power of 6,800 pounds, it will require the rapid combustion of 340 pounds of shavings, chopped straw, &c., to create a renewed ascent. Hence, including waste, probably about 100 pounds of fuel will be expended for every mile of conveyance, exclusive of the first inflation.

When this experiment has been made, it will be easy to try whether the balloon can be driven directly forward by sails wafted by the steam-engine at a less expense of fuel. In the former case the balloon had to proceed along two sides of a right-angled triangle only to gain the length of the hypothenuse: hence, as the resistance varies as the square of the velocity, the same horizontal speed of conveyance will be obtained with rather less than half the resistance in the one case than the other. The consumption of a steam horse power is about 30 pounds of water and six or seven pounds of fuel per hour. I have made several calculations relative to this subject, but it will occupy too much space for any one number of your valuable Magazine to detail them; I shall therefore close this paper, already perhaps too long, by stating, that if the dread of fire should deter any one from wishing to promote this experiment, notwithstanding the adoption of woollen cloth to prevent it, a perfect security from this accident may be obtained by using steam in lieu of heated air for inflating the balloon, or at least a great mixture of it with the heated air. The power of steam is greater than air at the usual temperature in Montgolfier balloons in the ratio of 18 to 11, although the first inflation will cost more fuel in the ratio of 2.6 to 1. The resistance to a steam-balloon will be only as 1 to 1.38, when compared with one of the same power inflated by heated air; and hence a considerable saving of power would be the result of adopting it. But several inconveniences arise upon the introduction of steam into balloons, the chief of which are the necessity of doubling the structure, so as to suspend the steam balloon within one of heated air or gas, and of the materials being incapable of absorbing water. However, I think it very possible that the following lines of Dr. Darwin may eventually be realised:—

> "Soon shall thy arm, unconquer'd steam! afar
> Drag the slow barge or drive the rapid car;
> Or on wide waving wings expanded bear
> The flying chariot through the fields of air.
> Fair crews triumphant, leaning from above,
> Shall wave their fluttering kerchiefs as they move;
> Or warrior bands alarm the gaping crowd,
> And armies shrink beneath the shadowy cloud."

I remain, sir, your obedient servant,

GEORGE CAYLEY.

Brompton, near Malton, Yorkshire,
Dec. 24, 1815.

II.

TILLOCH'S PHILOSOPHICAL MAGAZINE, Vol. XLVII., No. 217, May, 1816, p. 321.

TO MR. TILLOCH.

SIR,—I am glad to see the subject of aërial navigation noticed by Mr. Lovel Edgeworth, and likewise by a gentleman under the signature of T. H. in your Magazine for March last. Mr. William Bland has likewise written an ingenious paper upon this subject in the Monthly Magazine for March. I wish to bring all those who interest themselves in this invention, to act in concert towards its completion, rather than to be jealous of each other respecting their own share of credit as inventors: indeed, unless we can realise our object, very little credit will be due to these speculations. Mr. Edgeworth, who upon good grounds puts in so early a claim to a knowledge of the principle of steering balloons by the inclined plane, has quite misconceived the nature of the principle upon which my former papers upon the subject of aërial navigation were founded, when he says that "Sir George Cayley has frequently proposed to impel flying bodies by letting them descend obliquely through the air, and forcing them in a contrary obliquity against the air by *impelling them upwards*." My plan rested upon the following fact: that if a plane of any given magnitude, say 100 square feet, were placed so as to make any small angle with an horizontal line, suppose an inclination of one to ten; then, if it were propelled forward in the horizontal path, like a bird in the act of skimming, by a force of ten pounds, till the horizontal resistance of the air equalled this pressure, the plane would have power to sustain one hundred pounds weight during its progress. I proposed to create this slight horizontal pressure by the power of a light first mover, several of which were there alluded to, particularly one where the combustion of oil of tar was made use of as the moving power. This engine had been exhibited in a working state to Mr. Rennie, Mr. Cartwright, and other gentlemen capable of appreciating its powers; and it appeared from the minutes of Mr. Chapman, civil engineer of Newcastle, that eighty drops raised eight hundred weight the height of twenty-two inches; hence a horse power may consume from ten to twelve pounds per hour. The expense of power, however, with this engine being much greater than with the steam-engine, the patentee never proceeded further than the original experiments. From this statement it is plain that the tacking operation alluded to by Mr. Edgeworth,—by "*impelling them upwards*," and then letting gravity operate in the descent,—was not any part of my plan, although, being acquainted with the principle of oblique forces, and not having a proper first mover, I was obliged to try the experiments as to steadiness and steerage by merely allowing the vessel to sail from the top of a hill; and it was to this circumstance that I alluded in my observations upon Mr. Evans's paper, the descending half of the movement that gentleman proposed being perfectly similar to, and therefore corroborated by my experiments.

I have related this matter more minutely, because Mr. Edgeworth's statement rather gave me a claim to the principle of steering balloons by means of the inclined plane, which I do not possess. Indeed I hold it perfectly in my recollection, (although I cannot state either the name of the party or the publication in which I read the account,) that an ingenious young man several years ago had tried some successful experiments on the steerage of balloons by the inclined plane, but had died before he completed his invention.

Should this subject gain importance by experimental success, this, and no doubt many other claims of various kinds, will be attempted to be placed over the heads of those gentlemen, who now will, I hope, have the more substantial credit of realising and perfecting the invention for the uses of mankind. For my own part, I shall sue for no share in the scramble respecting Montgolfier balloons, but that of braving the risible muscles of my friends in substituting *acres* for *yards* of cloth in their structure, unless indeed it be for the addition of a *chimney*,

a most common, natural, and in this case particularly useful appendage to a fire. I shall however be extremely gratified in being able to claim the credit of standing on the same list with Mr. Edgeworth as a subscriber towards completing this noble art;—with this view I request Mr. Tilloch to be kind enough to permit a list of subscriptions to remain at his house till a few names be collected and a committee appointed;*—no one to be called upon for his subscription money till such a sum be subscribed for as will be sufficient to try the first experiment proposed by the committee.

In my last paper it was stated that I had made some calculations relative to the quantity of fuel that would be consumed in propelling the Montgolfier balloon there described, at a velocity of twenty miles per hour, by the steam-engine. According to the returns made of the Cornwall engines, Mr. Woolf's engine raises about fifty millions of pounds, one foot high, with a bushel of eighty-four pounds of coal. Hence I estimate that twenty pounds of coal per mile would be consumed in this operation, and therefore the expense would not be considerable.—But as no more than 6,800 pounds, the unconsumed power of this balloon, can be allowed for the weight of the engine, its water and fuel, besides that of an extensive surface for wafting, it appears upon estimating the weight of these things,† that this balloon, extensive as it is, is, as I before said, only the longboat of its species, and not quite large enough to take advantage of the saving power of the steam-engine at so great a velocity as twenty miles per hour: though it will answer the purpose of a first experiment at fifteen miles per hour with a proportionate saving of fuel.

Upon larger constructions, however, it will appear that aërial navigation will be performed much cheaper in a horizontal path by the steam-engine, than upon the ascending and descending plan with the inclined plane. Several years ago I made many estimates of the application of first movers to these long balloons; but I always found that the enormous size required to be successfully driven by them, placed the proper scale of experiments quite out of the reach of any individual, and unfortunately of such magnitude as to render the public of that day, if appealed to, more incredulous than ever upon the subject. My own experiments were therefore confined to the inclined plane, which offers a good result upon a smaller construction. The introduction of the steam-boat, together with "the steady march of the human mind" during so many years of unexampled scientific discovery, attended by practicable results in the rapid improvement of almost every art, will now enable me to state my ideas upon this subject, without stepping beyond the limits of many an inquiring mind. My former paper showed that the Montgolfier balloon there described was only the long-boat of this class of vessels; yet I felt obliged to pave the way for the introduction of so huge an infant obtruded upon the civilised world, by leading the unprepared mind from the contemplation of a hundred-gun ship of ninety yards in length, to a balloon of a hundred; and although in the course of this paper I shall be obliged to point out objects in reserve upon a larger scale, yet as a matter of experiment, I have nothing to add to the *bulk* of the balloon already described. It would be a great advantage to that vessel if the greater portion of its long chimney, from about twenty feet above the fire, were made of flexible materials well coated with the mixture of brick-dust, &c., with which the firework-makers protect their materials:—this would allow of proper movement when the machine was at

* I can have no objection to keep a list of the names of such gentlemen as may wish to contribute to an experiment of the kind recommended.—A. T.

† Coals for one hour ... ... 360 pounds
Water for do. 3540
Weight of the wing or waftage ... 1000
Leaving only ... ... 1900 for the weight of the engine.

6800

anchor to the earth from some point under the prow. It ought likewise to be observed, that, owing to the internal pressure there will be exerted a power of about 460 pounds per lineal yard, endeavouring to tear the cloth asunder. This power is easily provided against by a wide netting, although most cloths would sustain it without such additional strength. Another circumstance to be adverted to, is the advantage that would arise from regulating the shape of the prow in the best manner for obviating the resistance of the air. Theory unfortunately is of little use upon this occasion, for Nature, always true to itself, makes the prow of the *bird* concave, and that of the *fish* convex to their axes; whereas theory would appoint both alike. In the absence of all good theory, I shall, as proposed in one of my former papers, give the form of the woodcock from actual measurement, that bird having frequently to cross at least 500 miles of sea at one flight, and Nature seeming to have united every contrivance to blend strength with lightness in its structure;—hence it is more than probable, that as the resistance of the air was the obstacle which all this contrivance was calculated to overcome, the external form is nearly the best possible, being that which would more than all the rest tend to the ease of the performance.

Fig 2.

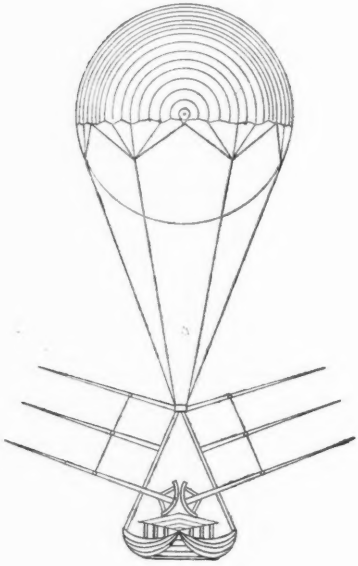


Fig 1.

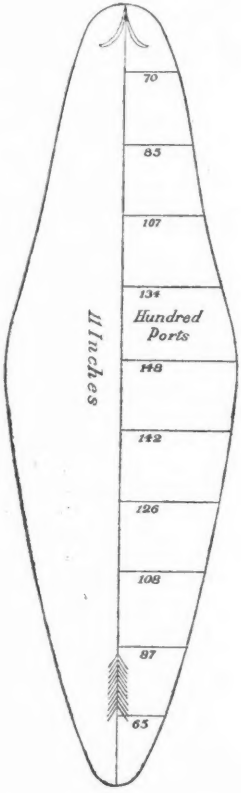


Fig. 1, Plate IV., shows a section of this bird through its axis. Fig. 2, is a transverse section with the apparatus suspended. The *ordinates* are given in hundredth parts of an inch at the distance of one inch from each other. The weight of the individual bird corresponding with these measures was 12½ ounces,

and the velocity of its leisure flight in calm air will have been about 28 miles per hour. It may possibly require a different form of prow for obviating resistance best at different velocities, and what may be the best form for a small object may not be so, even at the same velocity, for a large one; but notwithstanding this, I know no better guide, and shall therefore recommend the experiment to be made in the form of the prow delineated, as far as it can be made to agree with the flattened structure necessary in this instance for the trial of the inclined plane. That the practicability of constructing this balloon may be better judged of, I here subjoin the following estimates respecting it.

The quantity of cloth and the general appearance seem enormous; yet it must be recollected that it is only an inflated bag, and that the 5,750 square yards it consists of would all pack up in a cubical bore $3\frac{1}{2}$ yards each way, allowing 1-5th of an inch for the thickness of each fold, which must be amply sufficient. The cloth I made use of in my experiments generally weighed half a pound per square yard; but probably, including the netting, the estimate for the present purpose should be taken at a pound per yard; hence the weight would be 5,750 pounds. Let the car, chimney, &c., be taken at 2,000, and there would be the power of 3,050 left unoccupied, in addition to the power of 6,800 pounds for effecting its progressive motion: however, it would be necessary for one hour's travelling to carry about 2,000 pounds weight of fuel, which would only leave power to convey a crew of seven men: hence it would be more suitable to carry about 20 men, and to reduce the ascending velocity to 15 miles per hour, allowing the descent to remain at the full speed, which would reduce the general speed to $17\frac{1}{2}$ miles per hour.

This statement plainly shows that the Montgolfier balloon I have described is the least that would be efficient, which I trust will shield me from the imputation of holding extravagant opinions upon this subject in proposing it. Indeed the unwieldy bulk of these bodies is unwillingly thrust upon me by the result of calculations grounded upon the facts of the case.

The danger attending the hydrogen gas balloon, where any first mover is used that acts by fire, is a great obstacle to their introduction; otherwise a balloon of this kind equal in power to the one described, would not exceed 70 yards in length, and would meet with rather less than half the resistance, and of course not consume more than half the power to drive it at the proposed velocity—there are other inconveniencies attending these balloons, and their cost in filling is not one of the least. The great resistance upon the prow must be balanced either by a firm wanded texture, or by internal condensation*; the former is heavy and incapable of being folded up, and the latter wasteful, as it is impracticable to have it air-tight enough not to allow a vast escapage. This would oblige a double structure, one of thin oiled silk containing the gas, and one of a coarser texture surrounding it, which could receive the condensation necessary from common air driven in occasionally by a pump; or perhaps, with some little contrivance an aperture at the point of the prow, receiving the full direct resistance of the external air, commensurate with the velocity of the balloon, would answer this purpose. The danger from fire might be greatly reduced by having the balloon at a considerable distance above the car, say 20 yards, and the surface for waftage might be so arranged as to permit of a safe descent even in the event of the balloon taking fire, and being obliged to be cut away.

It was my intention to have ascertained what proportion of azote with hydrogen gas would render it incombustible on its access to common air; but owing to an accident in the experiment immediately previous to my leaving home, this must be reserved to a future opportunity;—perhaps the adulteration required may be so great as to render the specific gravity of the mixture too nearly the same as atmospheric air to be of any use in this instance; but if azote in the ratio

* [Cayley invariably uses " condensation " for " pressure."—W. L. M.]

of one half, or even two to one, be ascertained to be sufficient, it will be an excellent step gained towards realising this invention.

It is evident that if aërial navigation ever be brought home to the uses of man (and who, noting the progressive stages of society, can set a limit to the powers which the benevolent Author of his being designed him by proper gradations to become possessed of?), it can only be done upon a scale of which the balloon described is the first unit; and although in the present day this unit is abundantly large enough in a *practical* point of view to occupy all the attention that can be afforded to the subject; yet I shall nevertheless obtrude a little in *prospect* upon the duties of our posterity respecting this art, as in a doing (*sic*) the capabilities that remain in store for our race will operate as an encouragement towards our availing ourselves of the first step. It has already been observed, that as balloons increase in size, the diminution of their relative resistance, when compared with their power, keeps pace with that increase. Upon this principle it is easy to show that a balloon of the form delineated, inflated with hydrogen gas, when extended to the length of 144 yards, will meet with no more resistance when compared with the weight it will sustain, including its materials, than the resistance of the bird compared with its weight. Surely we cannot wish for a better basis for swift aërial navigation, than that of a vessel capable of suspending in the air as many multiples of the weight of the bird, as its resistance contains multiples of the resistance of the bird at the same velocity. This balloon would require 11,880 square yards of cloth, which would fold within a cubic chest rather exceeding four yards each way, if of single structure; but I have taken it as a double structure, which would of course require two such spaces, and the weight at two pounds per yard will be 24,000 pounds. I allow 17,000 pounds for the other materials, including a surface of the necessary extent for waftage; when, deducting these weights, the supporting power, which is 163,000 pounds, will be reduced to 122,000 pounds, or 50 tons. This power remains to be divided in a proper ratio between the weight of the first mover and that of the cargo or crew intended to be conveyed. Although two-thirds of the weight of most birds is devoted to the muscles of its wings, and arguing from the rapid consumption of their food, that their muscles may be more energetic than those of quadrupeds, weight for weight; yet it is improbable that this excess is in any great proportion. The famous race-horse Eclipse is said to have gone for one mile at the rate of 60 miles per hour, which is a far greater velocity than any bird whose flight I have measured; and he had the disadvantage of carrying the weight of a man upon his back. If therefore the energies of birds be taken at double those of quadrupeds in this statement, more than an ample allowance is made in their favour. This power in birds is chiefly employed in the waftage necessary for their *support*, and a smaller portion of it is applied to overcome the direct resistance of their body: but in the case of the balloon the *supporting* power is already obtained, and the whole energies of the first mover will be directed towards overcoming the resistance of the prow only, which is in no greater proportion to the weight of the whole apparatus than that of the bird to its weight.

Upon estimating the probable weight even of steam-engines upon a large scale, it appears that 160 pounds per horse power is an ample allowance with its load of coal and water for one hour;* and as the water is a considerable part of

* *Estimate of the Weight of an Expansive Steam-engine of* 100 *Horses Power.*

	lbs.
Weight of a cylinder 2 ft. 4 inch. diameter by 4 ft. 7 inch. in length one inch thick	1210
400 feet surface of tubulated boiler at ten pounds per foot	4000
Connecting parts, piston, &c. say	2000
Water for an hour at 30 pounds per horse's power	3000
Coal for ditto at five pounds per ditto, being more than sufficient in Mr. Woolf's engines ...	500
Weight of fuel constantly occupying the fire-place, say	1500
Weight of water occupying the boiler	3000
	15210

The pressure is estimated at 20 pounds per square inch, and the stroke 4 ft. 7 inch. per second.

the whole, and can be recovered again for the use of the engine by permitting the steam to pass within the double coats of the balloon, and to be thus exposed to so extensive a cooling surface, it is probable that 200 pounds per horse power will be more than sufficient for working twelve hours without any further supply of water or fuel—with a velocity of 20 miles per hour in calm air, this length of time would imply a stage of 240 miles. The steam horse power raising 550 pounds one foot high per second, is considered by engineers as exceeding the average power of the largest dray-horses one-fourth. Upon weighing one of these animals of about the middle size, I found it to be fourteen hundredweight or 1,568 pounds; and I am informed some of the largest of these horses have weighed a ton.—But if we take the smaller weight as approaching the average, we must increase it one-fourth part to make an animal equal in power to the steam-horse: hence the weight of the steam-horse will be as 200 pounds to 1,950 pounds when compared to that of the living animal. And to follow the argument I have been using, if the weight of the steam-horse be deducted, there will remain 1,750 pounds, which will, according to these estimates, be the weight of inert cargo in terms of horse powers which would be conveyed by this means. This is not a correct mode of estimating the matter in question; but I wish to show, by entering into this comparison of animal and engine power, how very much the latter exceeds the former in energy, weight for weight; and that even if the estimates should be so false as to be five-fold wrong, yet there would be sufficient power for the purpose. I shall deduct three times the weight of the engine estimated in the note from the 50 tons of power unoccupied in the balloon, when there will remain rather more than 34 tons of power for any purpose required. This would convey 500 men during one hour, 410 men during 12 hours, 290 men for 24 hours, and 50 men for 48 hours, without fresh supplies of fuel or water. The extent of the voyage in calm air would in the latter case be 960 miles.

I do not offer this statement with any expectation of its being realised in our age; but I do affirm that balloon navigation does hold out the capabilities I have so daringly ventured to investigate; and I trust that others will join Mr. Edgeworth and myself in promoting experiments upon a subject that promises eventually such advantage to mankind; the progress of civilisation being evidently commensurate with the facility of communication. The expense of constructing the Montgolfier balloon described in my former paper, if taken at three shillings per yard including the machinery, would be 870 pounds, and probably 1,000 or 1,500 pounds would defray the expenses of the experiment. The double cloth of the hydrogen gas balloon at six shillings would cost 3,600 pounds, and the engines, &c., say from 6 to 7,000 pounds; so that this immense fabric, when compared to the large ships it seems to outvie, is not a tenth part of their cost. Their expense, however, when compared with their freight, will be about £300 per ton. The formation of hydrogen gas by the usual process is slow and expensive; but by keeping fresh supplies of iron borings red hot within an extensive cavity properly constructed, and passing steam through the hydrogen gas, may be supplied at a very cheap rate, and with considerable rapidity.

The vast strides which science of every kind has made within the last twenty years renders every advancing step more easy; and it appears to me that England may soon have the honour of perfecting the construction of balloons, although the invention of them was not altogether her own. The heavy expense, however, of such extensive experiments renders it necessary that it should become a national object; and as the means of directing balloons are within the scope of information of almost every engineer in this mechanical country; the only real credit that can be acquired in this pursuit, will be given to those who do actually realise the invention, and this will be due to each individual in proportion to the liberality with which he supports the undertaking.

XUM

Large individual subscriptions however are not necessary to obtain £1,500 and in fact subscriptions of *one* guinea are often more productive than those of a *hundred*. But whatever gentlemen may think proper to subscribe, whilst the subject is before them, I would request them to give notice of immediately, as by that means, and *that alone*, can any further step be taken before the subject will be forgotten.

I remain, sir, your obedient servant,

GEORGE CAYLEY.

Brompton, April 5, 1816.

P.S.—The sketch represents an end view of a hydrogen gas balloon with three tiers of wing to be worked by the steam-engine, or any other first mover; each wing to be divided into many stripes or portions, which are so constructed as to heel up and down by the alternate pressure of the air above and below them at each stroke of the engine, and thus by their oblique waft to propel the balloon.

III.

TILLOCH'S PHILOSOPHICAL MAGAZINE, Vol. L., No. 231, July, 1817, p. 27.

To MR. TILLOCH.

SIR,—Since my last paper on Aërial Navigation, several scattered observations have been made upon this subject in your Magazine; and although it has not met with all the encouragement it deserves, yet it has received as much notice as can reasonably be expected, when it is considered that it invites its supporters to a subscription, during an unparalleled period of public pecuniary privation. I am glad to find that a gentleman of distinguished literary and scientific reputation has stated to you his intention of subscribing fifty pounds towards any experiments on this subject, that may be conducted by men of science; alluding, I conceive, to the committee proposed in one of my papers. Mr. Evans has likewise signified his intention of subscribing, in conjunction with Mr. Lovell Edgeworth* and myself. It therefore becomes necessary to publish the present amount of the subscriptions, which I propose, subject to the permission of these two gentlemen, may be done in your Magazine for July; by which time I hope a few more names may be added, and a fund for experiments on the improvement of balloons be commenced, which will in time enable the capabilities of this interesting invention to be properly investigated and ascertained, under the inspection of a committee of scientific persons, acting with the advice of the best professional engineers in the country.† Surely, when it is considered that this leading discovery of suspending heavy bodies in the air by balloons is but recent in our age; and that the cumbrous and expensive nature of their structure has placed the proper scale of experiments far beyond the expense that individuals choose to appropriate to such purposes,—it cannot be deemed absurd, or even unworthy a sense of national pride, by a combined effort of intelligence and contribution, to rescue this noble invention from for ever remaining a gaudy bubble in the hands of exhibition-makers. All that I ask of men of information upon matters of this nature is, to combine, and to try such rational experiments, as would show by degrees how far it is practicable to guide balloons:—such a committee as I propose would never enter into any of those projects which, whether ultimately false or true, are at present too many steps in advance to be proper objects of their immediate attention; but, commencing with what has

* Sir George will have learnt by this time that the gentleman whom he here names is now no more. He was the gentleman who had agreed to subscribe fifty pounds.—EDIT.

† I stated last year to Mr. Tilloch the amount of my subscription, as the original promoter, under certain conditions; for the present I shall say 50 pounds; but I by no means wish gentlemen disposed to forward experiments on this subject to subscribe upon a high scale, as a greater amount may probably be obtained in subscriptions of from one to ten pounds.

been ascertained upon this subject, would advance step by step from that point, as far as the present state of our knowledge of first moving powers will permit.

The title and terms of the subscription I therefore propose to be as follows:—

WE, the undersigned parties, enter into the following subscription, for the purpose of ascertaining how far the principle of balloons supporting heavy burthens in the air may *be made* useful as a means of conveyance.

No person to be called upon for his subscription money till at least £1,000 be subscribed for.

When the subscription has reached this amount, an annual committee of seven of the subscribers to be elected;—every subscriber of one pound and of less than five pounds to have one vote on this and all other occasions. Subscribers of five pounds to have two votes; and subscribers of larger sums to have one additional vote for every additional five pounds they subscribe.

No experiments to be undertaken but by order of this committee, who may call in the advice of such civil engineers as they choose to consult.

An annual report of the application of the fund, and the result of the experiments made, to be printed for the use of the subscribers.

These regulations being the basis upon which the subscription is made, cannot be altered; but subsequent rules not militating against these, may be entered into at a general meeting of the subscribers, expressly convened for the purpose.

Having now stated my sentiments respecting the general bearing of this subject, I proceed to notice some remarks that have been made by others since my late papers. Mr. Evans has suggested as an improvement upon the triple tier of wing waftage by the steam-engine, that a rotary movement with oblique surfaces* will be preferable, on account of the continual loss of power which he conceives to take place in putting these surfaces into motion from a state of rest. This reasoning against reciprocating movements is in general perfectly correct, but in this case the maxim does not hold good. The whole power communicated to these wafting surfaces is applied in the commencement to overcome the *vis inertiæ* of the materials of which they are composed, and the gradually increasing resistance of the air. Towards the termination of the waft, if the movement be properly contrived, the momentum accumulated in these surfaces will prolong the effective waft as much beyond the time when the effort of the first mover has ceased, as will exactly restore the power absorbed at the commencement of the action. Thus the whole power will have been expended on the resistance of the air, and consequently in propelling the balloon.

There are several difficulties of construction which occur in rotative wafts; the chief of which are, giving firm support and communicating motion to the axis at the necessary distance it is obliged to be placed from the boat; whereas in the wing waftage the hinge is on the solid frame of the boat. The wing construction likewise offers an advantage of great importance,—that of providing, if properly managed, a safe descent in case of accident to the balloon. The chief advantage of the rotary movement is its uniform action. I think either construction may be made effectual, but I prefer the wing plan as the easiest for our first experiments. Mr. Evans may see in my early papers upon this subject, that revolving flyers had not escaped my attention: indeed, the first experiment I made upon the mechanical principles of aërial navigation, was successfully executed, though on a very small scale and by very simple means, upon this very plan.†

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* [*i.e.*, an airscrew.—W. L. M.]

† Nicholson's Journal for November 1809, p. 172.

Some very ingenious observations on the subject of aërial navigation are made by a correspondent in your Magazine for March, 1817. In the third paragraph, respecting the means of vertical motion, the plan of condensing air into a second balloon is adverted to as worthy of particular attention. This plan of increasing the specific gravity by condensation, and lessening it again by the escape of the condensed air, was one of the earliest suggestions of the balloon-makers; but, though founded on a true principle, is quite inefficient in practice. The elastic pressure of air increasing as its density, no cloth is able to bear the force required: for instance, if a cloth be capable of resisting a lineal tension of five hundred pounds to the foot, let a balloon twenty feet in diameter be constructed of this cloth; it will readily be found upon calculation, that only from seventeen to twenty pounds of additional air can be pumped into it before it would arrive at the proposed tension. Thus a huge impediment to motion would be added to the machine, besides the additional bulk of the supporting balloon necessary to carry the weight of this incumbrance, without gaining any efficient power to compensate for these disadvantages.

In the second paragraph, respecting lateral motion, it is observed that the tacking plan, though worthy of much consideration, is incapable of counteracting any considerable wind, "as a little calculation will show." I must here remark, that if your correspondent will honour with his attention my statement respecting a Montgolfian balloon constructed on the tacking plan, in your Magazine for March, 1816, and will recalculate the powers of that construction, he will find that the horizontal speed will be about twenty miles per hour in calm air; but he must not, as he proposes, consider the major axis as elevated in an angle of 45° with the horizon; but at an angle of 30°, which will be found to cause the path of the machine to be in the former angle; 15° or 16° being lost, in what is similar to lee-way in ships, according to the flatness of the top surface of the balloon. Although a velocity of twenty miles per hour will not overcome some winds, and would scarcely be at par with what Mr. Smeaton calls "very brisk" in his table; yet it would overcome what he terms "gently pleasant," at a speed of sixteen miles per hour; and what he terms "pleasant brisk," at about seven and a half. Very few days in the year have what is thus called very brisk wind, and it is even in this case 32 to 1 that it does not blow from that point of the compass which is the proposed direction of steerage. In most oblique cases the power of the machine will give a great command of diagonal steerage within the semicircle opposed to the wind; on either side it will be no impediment; and in the whole semicircle behind the wind it will add to the velocity required. Hence, as on most occasions a choice of time is left, winds will be of infinite use in aërial navigation, even should twenty miles per hour, in calm air, prove to be the limit to the velocity of these machines. The difference of the currents in the upper and lower strata of the atmosphere, it is well observed by your correspondent, will lend great assistance to the steerage of balloons, as will also the singular fact of their following the direction of rivers, which is probably an electric phænomenon, rivers acting like discharging rods by connecting the opposite electrical states of distant regions of the atmosphere, as is exemplified by the greater frequency of accidents from lightning on their banks than in ordinary situations.

In the third paragraph your correspondent states the failure of oars in moving balloons to have arisen from their being applied to the car, in lieu of "their line of pressure passing through the centre of pressure of the whole system," much of the power being thus applied towards communicating a rotary movement of the car round the balloon. I do not conceive this to be the cause of failure, but the application of the power of one or two men, with very ill appropriated means, to perform what required the strength of twice as many horses. With respect to the oblique force noticed by your correspondent, I wish to refer him to the case of a barge drawn along the centre of a canal by a rope

to a horse on the bank;—no power is lost by this mode of draft, but what arises from the actual path of the vessel not coinciding with the line of its major axis, which slight increase of resistance is foreign to the case of a spherical balloon, where simple gravitation, and not the pressure of a fluid on an oblique plane, is the restraining force. This is best explained by a figure.

Let A, Fig. 1, Plate I., be a balloon. B its car, propelled beyond the centre of suspension by any given power of waftage; draw AC perpendicular, and CB parallel to the horizon; and let these lines be in the same ratio to each other as the weight of the car is to the propelling power; then the line AB will represent the whole action of the car upon the balloon. Draw AD and BD, respectively, parallel to the two former lines, and it becomes evident that the power of the compound force AB, will have the same effect as the two forces AD, equal to CB, the propelling power, and AC the weight of the car; which being just balanced by the floating power that may be represented by BD, leaves the balloon to be carried along in its horizontal path by the same force, as if dragged in the direct line of its centre AD. I have been the more particular in my observations on this point, because I wish to show that long balloons filled with hydrogen gas may be made use of at any distance above the car they support, which may be found to render them safe from the fire of the engine, and yet not be subject to any loss of power from the waftage being applied to the car in lieu of the balloon. Thirty or forty yards, if necessary, may intervene between the balloon and top of the chimney of the fire which works the engine. Wire-gauze, so celebrated of late for preventing the communication even of explosive mixtures of hydrogren with each other, may interpose its magic web to cut off any danger in this respect; and as the hydrogen gas balloon must (for the sake of firm resistance to the external air, so as to preserve the proper form of the prow) be inclosed in one of coarser materials, into which common air can be pumped to the required density between them, it becomes almost impossible that any accident from fire can take place. A flexible leather tube and cordage will thus form the only connection between the boat and the balloon. The stupendous bulk of such balloons as upon calculation appear capable of being made to convey considerable burthens with the requisite degree of speed, forms the chief obstacle to their introduction. This causes the expense attending their structure and inflation, their tremendous power if assailed by winds, and the difficulty of disposing of them when not employed. The expense of structure would at present be about £300 per ton; but if these vessels became of general utility, a much cheaper means of structure would probably soon be found out. The expense of inflating them with hydrogen gas is heavy by the present process; but as water consists of rather more than a sixth part of its weight of pure hydrogen;* and as every portion of hydrogen according to its purity gives from ten to twelve times its own weight of support in a balloon, it follows that every ton of water that is decomposed for this purpose, will suspend very nearly two tons of burthen in the air. If this process, as I before suggested, be performed by exposing red hot iron to the action of steam, it appears, from the known proportion of oxygen in the black oxide thus formed,† that it will take about a ton and a half of iron to each ton of supporting power; and hence an oven of three and a half yards cubed will contain sufficient iron drops or borings, allowing one half of the space for the free passage of the steam amongst them, to inflate the balloon I have described of fifty tons power. As the oxide will be reduced by melting the iron again in the ordinary way, no metal would be lost; and the process would not be expensive if conducted where coal and iron ore are found together, as is frequently the case in this kingdom.

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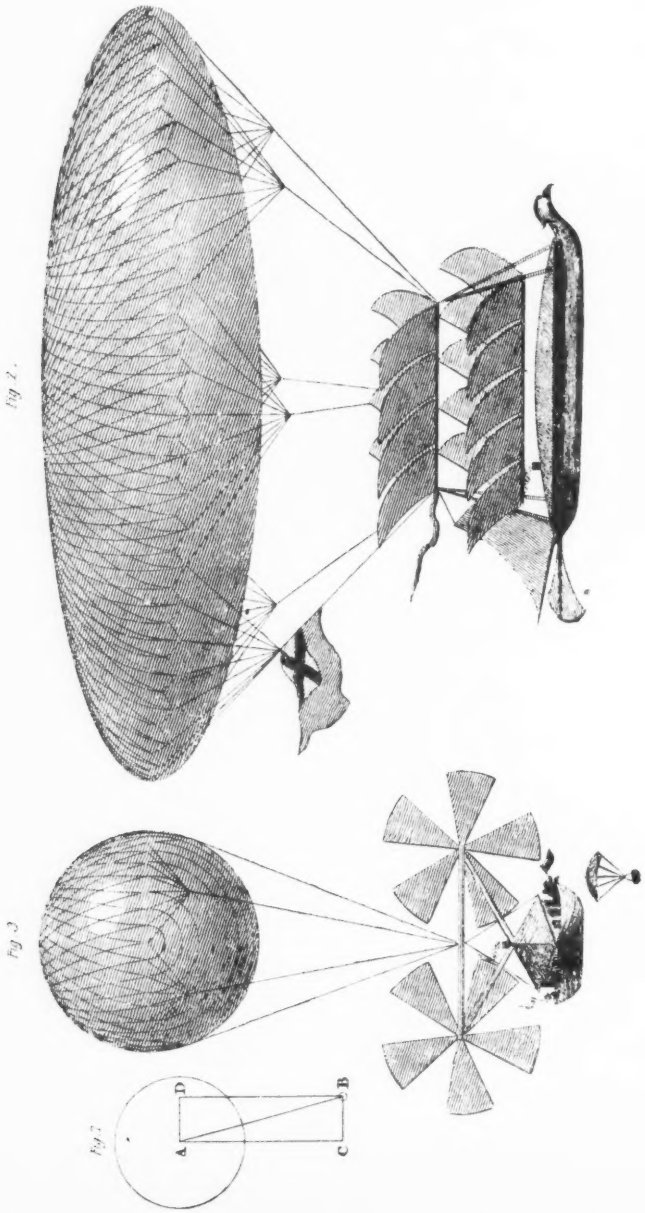
* 85 Oxygen.
15 Hydrogen.
100 Water.

† 27 Oxygen.
73 Iron.
100 Black oxide.

Charcoal will decompose water more rapidly and at a cheaper rate; and although the carburetted hydrogen thus obtained is generally much too heavy for inflating balloons; yet as the compound nature of this gas seems to vary according



to the quantity and circumstances under which the steam comes in contact with the ignited charcoal; and as Lavoisier and Meusnier obtained it at the specific gravity of 0.279, air being .1000, or rather more than three and a half times lighter than air, it is very probable that some ready mode may be found of obtaining pure hydrogen by the simple action of combustibles upon steam, which will render the floatage of balloons cheap enough for that ordinary use which, sooner or later, this principle was designed to be of to mankind. Had hydrogen been a scanty substance, to be found with difficulty, its remarkable levity, though attractive as a matter of curious chemical research, would only have been tantalising, as exhibiting a means of suspending heavy bodies in the air; but who will deny that in chemistry, as in every other branch of natural knowledge, there exist palpable evidences of design and adaptation, either of man to these elements, or of the elements to the uses of man. I do not here allude to those self-evident and immediate adaptations, such as light to the eye, the structure of the lungs to the air we breathe, or of the stomach to the water we drink; but those more indirectly adapted to the pleasures, wants and conveniences of life: for instance, iron, which is certainly the most useful of the metals, is the most plentiful; its power of being made into steel for tools, capable, by so simple an art as that of being suddenly cooled, of acquiring any degree of hardness, so as even to cut steel itself;—the extraordinary power it has of becoming so far in a state of fusion as to admit of being perfectly united under the hammer in a welding heat, without losing the form it had been previously wrought into, are, in the opinion of every enlightened workman, evidences of design in its chemical structure as respecting the wants of mankind. No one can doubt that water, which seems to form the basis of all the vegetable and animal juices, was likewise designed as furnishing the means of navigation. Nature is no niggard of that which she designs for the uses of her creatures. The sun, in lighting up our enamelled acres, far outdoes the utmost brilliancy of our nocturnal ball-rooms; and to hire an acre of illumination equal to what this luminary bestows upon it gratis, would cost from thirty to forty thousand per annum. The very circumstance that every ton of water contains a power of giving two tons of floatage to heavy bodies within the atmosphere, is strong evidence that this may be intended as one of the uses of the chemical arrangement of this plentiful element.

The relative power of balloons to break away from their anchorage in a storm of wind, decreases under the circumstances of magnitude and oblong structure I have proposed, in the same ratio with the decrease of their resistance in passing through the air. The horizontal drag of the balloon of fifty tons when at anchor, and exposed to the various degrees of wind in Mr. Smeaton's table, will be as follows:—

	Miles per hour.	Tons.
High wind	32½	8
Very high wind	42½	13½
Storm or tempest	50	19
Great storm	60	27

Hence, even in the great storm, if the boat be anchored to the earth, the wind would only cause the connecting ropes to incline back to an angle of 33° with a perpendicular, and by no means overcome the floating power and beat the balloon to the earth so as to endanger it; provided the strength of the materials were such as to bear intense condensation sufficient to preserve the form of the prow under this load of pressure. This necessity of balloons to bear considerable internal and external pressure will oblige these machines to be made of strong materials, and to be braced by a wide net of cordage. It will likewise be necessary to make them in several compartments, like the stomachs of a leech, the power of the same cloth to resist condensation being inversely as the diameters of the containing bag. This additional weight will of course in the same degree

diminish the supporting power: however, it may be practicable by means of tubes to each compartment, the mouths of which open externally to any required portion of the whole direct resistance of the wind, so to proportion the internal pressure, as only slightly to exceed the external in these respective compartments, and thus much of the strain may be avoided. The pressure of the atmosphere upon the skin of a moderate sized man amounts to about eight tons; but being balanced by an internal elasticity of equal amount, his lungs play without difficulty, and no strain is felt on any part of his skin. The necessity of having several compartments in large balloons, though an evil as to weight, is fully compensated for by the additional security it bestows:—by this structure, an accidental rupture of one portion would not cause a precipitate descent, as the floatage may be restored by a commensurate discharge of ballast, or of goods, in case of personal danger to the crew. The front or prow portion may be made of the strongest materials, and the hinder and middle portion of those duly proportioned to the stress they have to sustain; whereas, if all the air be in one vessel, every part must be alike capable of bearing the strongest strain. I would not have entered so minutely into these points, so much in advance of the present experimental state of the subject, were it not that the reluctance that is felt by some persons to aid experiments upon balloons, arises from a hasty conviction that the difficulties attending this subject are so great as to preclude all hopes of ultimately overcoming them: I wish to allow all the obstacles their fair weight, but to meet them by such expedients as their nature permits of, in doing which I fear I may have already trespassed too much upon your pages; and shall therefore conclude this paper with a very brief enumeration of the leading points that ought to induce experiments upon balloons to be made. They offer a direct swift and easy flotage from any one point to every other on the face of our globe. Their relative resistance decreases inversely to their power of support; so that the large balloon of fifty tons formerly described, will meet with no more resistance than the bird from which its form is taken, weight for weight. Every ton of decomposed water gives two tons of floating power. They would keep aloft, and be firm and steady in their position under anchorage, even in storms. The large balloon described, would pack up when out of use in a chamber within the boat eight yards by four, and thus render the apparatus compact on shore; and in the atmosphere there is unlimited space to accommodate any bulk with equal ease, especially when it is considered that every increase of it implies an increase of levity, and not of weight. Their structure being double, like a leathern football containing a bladder, the thin silken bag of hydrogen would not be exposed to any violence; and this gas being compressed on all sides alike by the condensed air surrounding it, would have no tendency to escape, during the action of the wind on the prow, as it would in the case of a common balloon, if at anchor or swiftly impelled through the air.

Danger from fire may be nearly excluded by the proper precautions. The same power that creates their progressive horizontal motion will effect their elevation and depression, by the application of an horizontal rudder or sail, and their steerage to either side by a vertical one. This will easily be understood from the sketch, Fig. 2, Plate I., which represents a side view of the arrangement of the moving and steering sails of a balloon on the wing plan. Fig. 3 represents an end-view of a balloon with rotary flyers. Neither of these sketches shows any of the connecting parts belonging to their movements, which would have made the drawing confused.

I remain, sir, your obliged and obedient servant

GEO. CAYLEY.

Brompton, May 12, 1817.

THE USE OF THE WIND CHANNEL TO AIRCRAFT DESIGNERS

A technical discussion on the use of the wind channel to aircraft designers was held in the Library on Thursday, January 8th, 1925, when the following were present :—Wing Commander T. R. Cave-Browne-Cave (in the chair), O. T. Gnosspelius, Griffith Brewer, W. O. Manning, C. C. Walker, H. N. Wylie, G. T. R. Hill, A. P. Rowe, A. A. Hammond, B. W. Townshend, R. Reynolds, G. P. Douglas, J. S. L. Oswald, W. H. Sayers, T. M. Barlow, R. McKinnon Wood, N. A. V. Piercy, V. Gaunt, R. K. Pierson, F. L. Richards, A. H. Tiltman, S. H. Evans, N. S. Norway and A. V. Roe.

In opening the discussion, Mr. C. C. WALKER said that the wind channel should be a means of supplying a mass of systematic data which will indicate the direction in which promising full-scale experiments may be made. As regards channel tests on a complete model of a proposed aeroplane, these seem to be of small value, the ordinary aerodynamic qualities of the model being completely masked as a rule by the interaction of propeller and body ; and even if the conditions with propeller running could be made completely similar, there still remain features which may be of the utmost importance on the actual aeroplane, which are either not observable at all or do not obtrude themselves as important in the channel.

One may say that no considerable departure from normal practice should be made on the basis of tests of a complete model, but such a departure should be based on some kind of full-scale experiment.

Before discussing the value of channel tests, he would like to illustrate the negative statement just made by a few examples.

The first D.H.10 was below its estimated top speed. It was a twin-engine machine with the engine nacelles between the wings. The engines were dropped right on to the lower planes in the D.H.10A, following some rough experiments, and the deficiency of about eight m.p.h. was recovered. Model tests made at the N.P.L. while this alteration was proceeding indicated no improvement in speed, but a slight improvement in climb. This illustrated the masking of ordinary aerodynamic features by slipstream phenomena.

In the D.H.29, a high-lift cantilever monoplane, there is some kind of instability in the flow at large incidence which entails a difference in force on the stick as between engine "on" and "off" of about 45 lbs. (There is some reason to believe that in the "engine on" condition there is an additional downwash associated with a lift coefficient much larger than that obtained in the channel, but nothing can be definitely known about this without further full-scale trials.) This illustrates that one may encounter a phenomenon of the most serious importance, which cannot be revealed at all by the channel tests on a scale model.

He then took two cases of features which are duly recorded by channel tests, but the importance of which there is no means of appreciating without tests in the air. They are both connected with means—whether by special devices or by high-lift wings—of obtaining high lift. Such devices increase the range of angles through which the wing may be used. This may have a very great effect on the control and flying qualities, and may make it difficult to use the high-lift end of the scale.

High-lift devices also necessarily have a high induced drag. This involves a rapidly deteriorating L/D as the stalling angle is approached which tends to

steepen the flight path just when it is desired to flatten out. The mixture of these two effects may result in tricky and difficult landing.

Direct disagreement between channel and full-scale measurements seemed to be very rare, and may be due to some accidental circumstance. There is probably a disagreement in stalling speeds, which are generally lower on the full scale. He thought the only example of clear disagreement we have ever come across is the difference of attitude between R.A.F.15 and Airscrew Section No. 1 of R. & M. 322—the attitude referred to being that of the chord to the horizon. On the channel tests there should be little or no difference. When measured in the air there is a difference of $7\frac{1}{2}°$. Needless to say this was so large as to be noticed at once before measurement. This difference has been noticed on another entirely different type of aeroplane, on which the two different wing sections were successively used.

These few examples would explain the view expressed at the beginning—that the wind channel is not a substitute for the full-scale test.

The utility of the channel as an instrument of research is obvious. It should supply such systematic tests of all kinds of arrangements that anything the designer wants to try on the full scale is likely to be a sort of interpolation.

The tests on aerofoils given by Dr. Junkers in his paper before this Society were, he thought, typical of the sort of information the channel should give—first systematic tests of all sorts of variations in the camber of the mid-line, then taking one camber and trying the effect of successive increases of thickness, and covering extremes in both directions.

Any such investigation should, of course, be run through at one speed and scale, in one channel and at one time. If this were not done their value would be greatly reduced.

Further examples of the kind of investigations wanted are :—

Interference with slipstream.
Various obstructions in different positions and so on.
Biplane interference of wings, having an increasing value of the maximum lift coefficient.

Many others might be suggested, especially as much of the comparative information which is available consists of tests separated by years and carried out under entirely different conditions. Such results are only misleading.

These and other kinds of abstract research are not well suited to aircraft constructors who have, sooner or later, to recover the cost of it in the price of their aeroplanes. This sort of information should emanate from national laboratories and be available to all constructors.

His firm had come to the conclusion that any funds which are available for experiment are best devoted to full-scale work direct to the point, which, after all, need not be very costly.

To sum up the situation, one might say that, provided there is a good output of generalised tests and research from the national laboratories, and provided the designer can do a certain amount of full-scale work, the wind channel may be looked upon as a luxury rather than a necessity.

Mr. W. O. Manning said that he agreed very largely with Mr. Walker. He did not know how many firms had wind channels, but he saw no reason to believe that their products were appreciably better than those who had not. A channel was of use for testing certain parts describable as "odds and ends." It also might be necessary to test two alternative forms of a part suggested in the channel in different ways, but it required an expert to correct for the differences caused by the different methods of support that might be necessary. Only results at the highest possible $V L$ were of interest owing to scale effect.

Major BARLOW said that as a representative of a firm without a wind channel their chief trouble was to obtain use of one of the N.P.L. official seven-foot channels for test of scale models, as these were full up with pure research usually six or seven months ahead. This meant that they could not take advantage of the information to be gained from wind channel work as to stability and control before an experimental machine was flown. He further asked for information as to the relative corrections for different sizes and types of tunnels and if these were definite. He also referred to the desirability of obtaining photographic records of air flow in channels round the test models.

Mr. R. MCKINNON WOOD was inclined to take a different view from the previous speakers, possibly because he was intimately connected with wind channel work. He could not imagine himself as a designer content to work without a wind channel. In reply to Major Barlow, there were quite definite theoretical corrections (at given scale and speed) from four-foot to seven-foot channels and to free air. A long series of experiments had shown that there was good agreement between model and full-scale tests on biplanes with R.A.F.14 and 15 wings. Visual records of types of flow were few, though from time to time they had been obtained by using threads and by other methods. Apparatus could be devised. Experiments had been made in Tokyo on patterns of flow round various bodies. In regard to Mr. Walker's criticism that the slipstream effect destroyed the value of model results, he wished to urge that wind channel technique should be developed in this direction. The N.P.L. were driving a propeller by an electric motor inside the fuselage in their duplex channel, and the same thing could be done for models of the smaller aeroplanes in a seven-foot channel. This was the ideal method, but it could not well be applied to models of large multi-engine machines or in a small channel. Slipstream effect could, however, be obtained by fixing the propellers in a separate frame ahead of the model. The thrust moment was not obtained, but this could be calculated and corrections made. The experiment was not particularly difficult or costly. He would point out to Mr. Manning that a number of designers who had no wind channels had tests made in the Government channels.

Dr. PIERCY was surprised at the views expressed of the limited use of channels. A feature of the Wrights' work that had always strongly appealed to him was that after their first year's gliding experiments they went back to the wind channel. Progress in construction and flying since that time was probably no greater than the corresponding development in model experiment. He could not conceive of an important change in aeroplanes being achieved without the use of channel tests. In spite of the handicap of low scale and an uncertainty that arose from time to time in connection with effects of the channel on the flow, definite information was usually obtainable from channel work.

He could understand those firms who did not require constant reference to model experiment hesitating to face the cost of a channel. The suggestion that data should be compiled in the Government laboratories to so unlimited an extent that designers would only require to interpolate between the results would involve a great expenditure of time and would therefore, if for no other reason, retard progress. There was a good case for relieving the national laboratories of such fundamental problems and detail work that did not demand a very large channel. A more general installation of channels in our universities would go far towards giving effect to a policy which should considerably increase the output of useful research.

Mr. PIERSON said that his firm had a channel, but he agreed that very misleading and dangerous results might be arrived at unless its use was properly understood.

The actual operation of a channel certainly required a high degree of skill,

while considerable judgment must be exercised when deciding just what tests to make, and he had found that the necessary skill and judgment were not arrived at without several years of experience.

Tests on various types of balance for control surfaces had not given good agreement with full-scale results, but his experience of longitudinal stability tests had been more satisfactory. He thought models should always be tested complete with propeller, but as they had only a four-foot channel, which made it impossible to put a motor inside the model, their practice was to mount the propeller separately from the model and drive it from outside the channel by a Meccano chain transmission—a method which sounded crude but gave good results in practice at very high revolutions.

In the matter of air flow round models, he thought that a somewhat unusual method of which they made considerable use might perhaps be of interest. A thin flat steel sheet is cut to fit the profile of the model in the position and plane in which it is desired to investigate the flow. This is then coated with a mixture of lamp black and shellac varnish, and, while still tacky, very fine grains of sand are scattered as evenly as possible over the plate. When dry the finished plate is held edge to wind in position round the model by wires and French chalk is introduced into the channel by a tube a little upwind from the plate. The blown chalk is then caught by the sand and forms two tiny diverging tails behind each grain, the mean direction of which is that of the air flow at the particular point. It has been found advisable, upon completion of the test, to photograph the plate if permanent record is desired, since the chalk is so easily rubbed off by handling. Localities of dead air or of eddies are shown very clearly by this method, which has proved of great help in eliminating unsuspected causes of resistance.

Captain HILL thought that tests on complete models were of great assistance. Aeroplanes could be, and of course were often, designed without model tests, but a lot of additional information of value was then lost. He had been successful in predicting the tail setting of the first Handley-Page W.8 from a model of this eighty-foot span machine tested in only a four-foot channel, but in this case they were not troubled with slipstream effect, which was of great importance. He thought channel work was decried largely because the results were not interpreted correctly; for instance, the results of a model test without slipstream were sometimes used to predict directly the behaviour of the actual aeroplane in flight with the engine running. He thought an intermediate size between the four-foot and the seven-foot would be useful, as it was very difficult to get accurate wing contours on small-scale models suitable for a four-foot channel, while a considerable saving in running expenses over the seven-foot channel would be shown.

Wing Commander CAVE referred to the value of a wind tunnel, which, although not constructed with the care and accuracy necessary to give accurate numerical results, did give indications as to general characteristics. A wind tunnel constructed at Kingsnorth during the war took its supply from an airship shed through a carefully-shaped mouth. The air was drawn through the tunnel and discharged into the open by the airscrew of an old aero engine of 150 h.p. The size of the working part of the tunnel was 12ft. × 6ft., and the speed and steadiness were surprisingly good.

Such subjects as the flow of air round airship sheds and screens, the balance of airship rudders, the steadiness of airship tail planes of new type, an aerodynamic relay for operating rudders and elevators, calibration of airspeed indicators against a standard were examined. It was now possible that a really large tunnel of the same "approximate" type might be constructed to investigate the stability of an airship approaching the mooring mast. In a tunnel of this type discharging into the open the air flow was well shown by fine streams of smoke.

Mr. R. MCKINNON WOOD said that a theoretical correction for the effect of

the constraint of flow by channel walls on trim had been worked out and confirmed by experiment and would shortly be available. The channel walls tended to flatten out downwash and so reduce the tail setting for trim.

Mr. REYNOLDS said he thought Mr. Walker had overlooked the question of cost of full-scale experiments for which accurate instruments must be provided. Including insurance, modifications, etc., it would cost about £25 an hour to put a machine in the air for full-scale tests. This sum would more than cover the cost of a channel for a week and the amount of data obtained from it in this time would be greatly in excess of the data obtained from one hour's flight.

Mr. BREWER said he was glad to hear Dr. Piercy's recognition of the pioneer wind tunnel work of the Wright brothers. It might interest Captain Hill to know the Wrights' later method of making models of aerofoils for testing in the tunnel. They used an aluminium base around which a wax model was cast. They cut upper and lower steel templates for both surfaces and used these templates for planing the wax down on both sides of the model. He sent Mr. Orville Wright an engraving machine from England, by which large-scale patterns of the section were reduced for milling the small-scale steel templates. Their original 1901 tunnel was sixteen inches square, in which they took comparisons by balancing the surface against the resistance of a wire grid in the tunnel.

Mr. HAMMOND said that a designer using a channel individually would see small points that would be overlooked in studying the results of others.

Dr. PIERCY said that the point raised by the last speaker was of some importance since there were often interesting details relating to a test which were unsuitable for inclusion in a formal report. As the Chairman had already pointed out, too, much information of interest to a designer could be gathered by approximate, or even rough, methods, where formal investigation would entail elaborate apparatus. At East London College, some success had attended the experiment of inviting a competent member of the staff of a firm to be present during part of a test—or even to conduct the experiment, under special circumstances.

Mr. A. P. ROWE said it would be useful if an opinion were expressed on the extent to which a compressed air channel, when corrected for wall effects, would give a complete solution of the scale effect problem for complete models.

Mr. R. MCKINNON WOOD said it would be quite possible to build such a channel in which models could be tested at a $V L$ corresponding with full scale. He had worked out a design. The theory of compressed air channels was as follows. The law of dynamic similarity requires that the value of Reynolds' number $V L/V = V L \rho/\mu$ shall be the same, where V is speed, L is scale, ρ is air density, and μ is viscosity and depends only on the air temperature. So in testing a one-tenth scale model at flying speeds we may obtain the full-scale value of the number by working in air at ten atmospheres pressure. The forces on the model per unit area are, however, ten times as great as full scale, and so is the power required by the channel. But by using a higher pressure with corresponding reduction of speed we may attain the full-scale Reynolds' number and reduce the forces and horse-power to tractable quantities, for the number is proportional to ρV, while the forces per unit area vary as ρV^2 and the power as ρV^3.

The question was whether the flow would become excessively turbulent and so discount the value of the tunnel due to the attainment of the full-scale Reynolds' number. He saw no reason why the turbulence should be greater than in the ordinary channel, if the design were equally good.

Mr. GNOSSPELIUS said he thought turbulence was an extremely important matter. The study of the flow of fluids in pipes showed that when turbulence

occurred the particles were not going in straight lines. He thought turbulence did occur in channels, perhaps not to an important extent with certain types of wing, but turbulence might explain the difference in the results obtained with high-lift wings. Did we really know what turbulence was? Was turbulence in free air the same as turbulence in the channel? He thought the Government laboratories might do valuable work in exploring this question which caused the only doubt he felt as to channel accuracy. Goettingen results showed a considerable fall (about one-third) in the resistance of a sphere at higher speeds than had been tried in this country. There was an alternative to the channel in the pendulum method which gave good results in resistance measurements but nothing like the same agreement in lift. He thought that all aircraft designers ought to use a wind channel.

Mr. McKinnon Wood said he defined turbulence as the presence of small eddies in air. A grid of round wires set up eddies in the form of vortex rings. If such a grid were set in front of an airship model it increased the drag but had no effect on aeroplane wings, though he did not know why. The Goettingen channel had a special form of entry with the honeycomb at the widest part which removed the source of turbulence virtually further from the model. This type was probably superior on that account.

Captain Sayers said there was an American Advisory Committee Report on tests of a set of thick wings in channels of different types. The maximum lift coefficient of this family of wings differed systematically by about twenty per cent. in the two channels. The report said that this was "obviously" due to the higher turbulence of the (N.P.L.) type of channel. The British experiment showing no change in an aerofoil characteristic as a result of increased turbulence was very probably made with R.A.F.15 or some similar section which was obviously insensitive to any small change in conditions. If the experiment were repeated with R.A.F.19, which evidently set up a relatively unstable form of flow, quite a different result might be found.

Mr. McKinnon Wood said that models tested in a closed-in channel gave a higher maximum lift coefficient than in the open type channel. The variation might be due to this cause and not turbulence.

Dr. Piercy said that introducing the subject of turbulence directed attention to a difficult problem that might not be solved for years. Possibly it was where the flow bordered upon a state of instability that the effects of the initial turbulence in the channel presented most interest to designers. Occasionally, at the scale obtainable in a 4-foot channel, the flow giving high lift would be so sensitive to external influences that tapping the balance, or even rapping the channel wall, would change it. In such a case the lift curve near the critical angle might be anything between certain limits. Well-known evidence led one to hope that further increase of scale would make a good form of lift curve stable, but the possibility of an exception had to be considered. Again, an aerofoil had been known to give substantially different maximum lift coefficients when tested in two channels of the same (N.P.L.) type, but of different sizes, the model not being of more than the conventional size for the smaller channel. Finally, the lift curve over an interval including the critical angle sometimes depended to an appreciable extent on whether incidence was increased or decreased during the test. With an aerofoil of this kind, the efficient type of flow could be nursed to yield a good maximum lift by slowly increasing incidence from a relatively small angle, while, once strong turbulence had set in, a considerable decrease of incidence would be necessary for a recovery. Thus, in the neighbourhood of the critical angle, the aerofoil would have two distinct lift-curves; one for increasing incidence, and another, with a lower maximum, for decreasing incidence—presuming always the change of incidence to be effected with the channel working. He wondered whether a dis-

appointment in full-scale maximum lift might sometimes be traced to oscillation of the aeroplane.

Mr. REYNOLDS mentioned that in the case of the Handley-Page slotted wing, Goettingen could never obtain such high lift coefficients as Handley Page, Ltd., could in their channel, the Handley Page results being confirmed on full-scale. This rather goes against the argument that an open type tunnel gives a more true representation of the maximum lift coefficient.

Mr. MANNING said that it looked as if a skilled operator could produce almost any results from a channel. Operation of channels was a very skilled matter and in unskilled hands inaccurate results might be obtained. It was doubtful if sufficient skilled operators were available.

Dr. PIERCY, commenting on a conclusion suggested by Mr. Manning, thought that remarks that had been made relating to difficulties in dealing with turbulence should not be reflected too strongly on the general question introduced by Mr. Walker. He imagined that intricate problems such as this would rarely be tackled by a designer for financial reasons.

Mr. REYNOLDS said that thanks were due to the N.P.L. and R.A.F. for doing the slogging in regard to precautions and corrections for wind tunnels. The manufacturers could then apply these corrections to their own results, making them both useful and reliable. Any other difference could be found by making a test and having the same model tested at one of the Government establishments.

Major N. WYLIE said that, speaking as one outside, it had seemed to him that the people who used wind channels at any rate had more information to give than the others. He thought that designers who had their own channels might learn a lot of things that the specialist might overlook.

Mr. WALKER said he had wanted to bring out the point that a change from a model fixed on a spindle in a channel to a full-scale machine free in the air was too great without full-scale experiments. A firm had only a certain amount of money available for experiment and in his opinion would do better to spend it on a method in which the aerodynamical qualities were included in the test. He thought that Mr. Reynolds' figure of £25 an hour was too high in view of the fact that his firm could hire out machines to the public for £8 or £9 an hour. [Mr. Reynolds interposed that he had included the cost of making the necessary modifications, etc.] They had changed the wings on one of these hire machines to obtain information and had therefore carried out an experiment while the machine was earning. He had not meant to give the impression that wind channels were useless. He thought that great results could be got from the tests in Government laboratories, but that results from tests carried out at widely separated times in different channels were difficult to correct.

Mr. A. V. ROE (*communicated*): Mr. Walker and those who have entered into the discussion have raised many interesting points, but after all is said and done, I am reminded of the old saying "An ounce of practice is worth a ton of theory." The wind channel up to the present can only be considered a useful stepping stone between the two. Let us hope with further research work, more valuable results will be obtained from the wind channel.

THE REACTION OF A STREAM OF VISCOUS FLUID ON A ROTATING CIRCULAR CYLINDER.

BY MAJOR A. R. LOW.

At the request of the Council the following notes on the subject have been prepared, with special reference to the Flettner "rotor ship" and to possible applications in aeronautics.

While it is necessarily somewhat provisional, the data available are pretty full, and it is hoped that no serious modifications will be required as a result of further experimental investigations.

In the now well-known Joukowsky transformation from a circle to a wing profile, maintaining the circulation G and the steady flow at a distance U, unaltered, the diameter of the cylinder $2a$, its angular velocity ω, the chord of the profile $4a$ (subject to a small correction of about -5 per cent) and the effective incidence β are connected by the relations $G = 2\pi a^2\omega = 4\pi a \sin\beta U$ so that $a\omega/2U = \sin\beta$.

Taking the projected area of the cylinder per unit axial length, and the area of the profile per unit span projected on the chord we get the lift

$$L = \rho G U = 2\pi a^2 \omega \,.\, \rho \,.\, U \text{ for the cylinder,}$$
$$= 4\pi a \sin\beta \,.\, \rho U^2 \text{ for the profile.}$$

The corresponding lift coefficients are $C_L = \pi a\omega/U$ for the cylinder and $\pi \sin\beta$ for the wing profile. The cylinder has half the projected area for the same circulation and therefore twice the coefficient. β is also the angle between the radii drawn to the dead water points on the cylinder and the diameter parallel to the flow at a distance. When $a\omega/2U$ is unity the dead water points become coincident and β is $\pi/2$; when $a\omega/2U$ exceeds unity there is still only one dead water point, but it no longer lies on the cylinder and Joukowsky's transformation fails. In correspondence with this failure it is impossible for $\sin\beta$ to exceed unity for real values of β.

It will be noted that the relation $a\omega/2U = \sin\beta = 1$ marks not only the limits of Joukowsky's transformation, but also the transition of the flow round the rotating cylinder from one state with two dead water points on the surface and

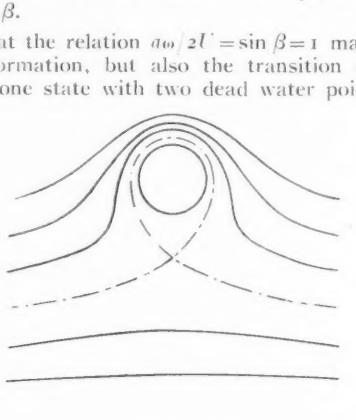


FIG. IV

no actual circulation to another very different state, in which elements of fluid actually circulate round the cylinder and the dead water point is at a distance below the cylinder (Fig. IV.).

Consider first the state where the analogy between cylinder and wing still holds good as far as the mathematical transformation is concerned.

It is well known from numerous experiments that the resistance of a circular cylinder not rotating is much greater than that of a good wing at zero incidence. Representative values of the drag coefficients may be taken as $C_D = .03$ for the wing and .8 for the cylinder. But at high values of Reynolds' number it may be much less.

At the best gliding incidence we have in a typical wing $C_L = .05$ and $C_D = .040$ so that $C_L/C_D = 16$ approximately.

If we assume twice the lift coefficient for the cylinder with the same circulation and no increase for the drag coefficient we get $C_L/C_D = 1.3/.8 = 1.6$, or only one-tenth of the ratio obtained for the wing.

No doubt some such reasoning as this was in the mind of every aerodynamician who was familiar with the Joukowsky theory and with the experimental data available, and accounts for the fact that no attempt was made to extend the range of observations. It remained for an engineer in search of novel applications, in particular the substitution of rigid wing profiles of good experimental shape for the conventional sails, to take up the idea of applying rotating cylinders somewhat by a lucky chance.

Experiments carried out at the Goettingen laboratory showed that within the lower range of $a\omega/2U$, corresponding to the flying range of the derived wing profile, the results were much worse than is indicated by the argument given above. The results are here taken from Flettner's paper at the general meeting of the Schiffsbautechnischen Gesellschaft at Charlottenburg on Nov. 21, 1924. The data in Technical Note No. 209 of the U.S.A. Advisory Committee for Aeronautics are also utilised. The German results have the great advantage of comparison with full size practical conditions.*

Both laboratories agree in the new and remarkable fact that at ratios of $a\omega/2U$ in the neighbourhood of zero, the lift curve does not increase with ω when U is constant, but touches the $a\omega/2U$ axis at the origin. Thus just where the wing is most effective and the lift and lift/drag are increasing fastest, the rotating cylinder does not give any lift at all. This result is so remarkable that it is worth while suggesting an explanation.

In the case of a wing at flying incidence, Prandtl explains the mechanism whereby circulation and consequently lift is produced as follows:—Looking at Fig. 1 the dotted contour, including the wing and a vortex at the wing tip just

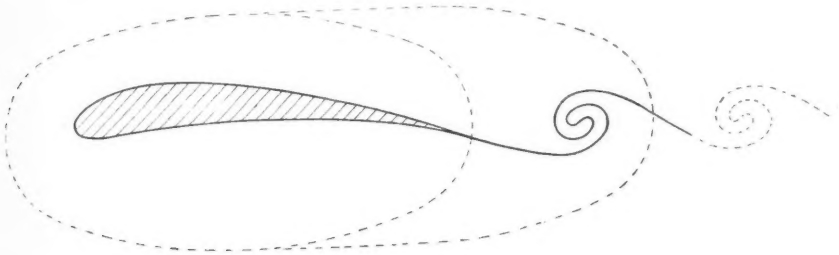


FIG. 1.

* Flettner's paper is summarised in *Engineering*, January 23rd, 1925, pp. 117-120, and "Technical Note" 209 in *Flight*, January 8th, 1925, pp. 17-20.

on the point of detaching itself is, according to the boundary layer theory, a line of no circulation initially. But there is palpably circulation set up round the vortex, therefore there must be equal and opposite circulation set up round the wing. As the vortex is detached and is carried with the stream it ceases to exert sensible effect on the conditions near the wing and the circulation established round the wing begins to decay by dissipation of energy. Presently, however, another vortex is formed and detached and this may be supposed to make up the circulation lost by dissipation since the shedding of the previous vortex. Thus the circulation round the wing oscillates between a maximum when a vortex has just been detached and a minimum between successive vortex detachments. This explanation falls in with Joukowsky's hypothesis that just enough circulation is required to bring the dead water point to the sharp tip of the wing. It also suggests that between the detaching of successive vortices the circulation may fall below the value required by Joukowsky.

Now consider the flow in the neighbourhood of a rotating cylinder in the stream. In Fig. 2 there is no rotation and it is known experimentally that equal vortices of opposite vorticity detach themselves alternately (in some condi-

FIG: II.

tions, simultaneously) above and below. By Prandtl's reasoning we may take it that there is an alternating circulation round the cylinder, opposite in phase to the vortices detached alternately. The mean circulation over a cycle or over a long time is evidently zero. When the cylinder is given rotation about its axis, as shown in Fig. 3, the velocity is in the first place increased on the upper side and

FIG: III.

decreased on the lower side, and this is necessarily accompanied by circulation in the sense of rotation. As a secondary result the vortices detached on the upper side may well be more vigorous than those on the lower side. The mean vorticity due to the shedding of vortices is therefore not zero but has a finite value in the opposite sense to the circulation set up directly by the rotation. The very interesting experimental result thus seems to be established that for small ratios of $a\omega/2U$ the two effects, one due directly to rotation, the other to the vortices detached, exactly balance, and there is no resultant circulation.

The American results give a much larger number of test points and show the curve of lift against $a\omega/U$ as coinciding with the axis up to a value of nearly .5 and then leaving it at a finite angle, while the German results have a smooth curve drawn through the test points and touching the $a\omega/U$ axis at the origin. If the American results are to be taken as accurate, then the neutralisation of the circulation due to the rotation of the cylinder by the counter-circulation due to the asymmetry of the vortices thrown off, is a sharply defined phenomenon up to a value of $a\omega/2U$ nearly equal to the stalling point of the corresponding wing profile. This completes the discussion of the statement above that the rotating cylinder is much less effective than might reasonably have been assumed from superposing the effects observed when it is rotating without translation and when it is in a stream without rotation, so long as we remain within the flying range of the derived wing profile.

According to both reports, when $a\omega/U$ = unity the lift coefficient is already unity also, beyond this point the lift curve, plotted against $a\omega/U$, rises more rapidly.

The American tests, however, are for infinite aspect ratio only, while the German tests are given for the same finite aspect ratio, indeed, but with end shields fitted which act as side curtains for a wing and thus give several effective aspect ratios.

Prandtl has shown that a biplane with side curtains is superior to one without, in so far as lift and induced resistance are concerned, but in practice the form and tangential resistances of the side curtains produce such an increase of drag that there is no net advantage. In this case the circulation is small compared with the flying speed so that the end effect is also small for ordinary aspect ratios. But with a cylinder rotating with a ratio $a\omega/U$ above unity, the circulation velocity becomes greater than the stream speed, and centrifugal forces set up serious transverse circulation. Now the velocity of circulation falls off with the radius, in the case of an infinitely long cylinder, and the centrifugal force therefore as v^2/r, *i.e.*, as G^2/r^3, and by fitting the cylinders with a circular end shield of twice the radius the centrifugal force at the ends is reduced to 1/8th of the value without shields. The effect of the circulation in axial planes is to bring fresh air, not in rotation, flowing in at the ends before the cylinder has time to bring the air already in its neighbourhood up to full circulation by diffusion of velocity in accordance with the equations of viscous fluid motion.

In this way the effective circulation, and the lift in consequence, is seriously reduced, and the advantages of the end shields now outweigh the additional head resistance. This is shown in the clearest possible manner by the comparative for a model cylinder 70 mm. diam. by 330 mm. axial length without end shields, with end shields of 120 mm. diameter, and finally with end shields of 140 mm. diam.

The fitting of the 120 mm. end shields more than doubles the maximum lift, while the increase to 140 mm. gives an additional increase of about 10 per cent.

It is thus fully established that the inertia forces due to the vigorous circulation round a cylinder of which the peripheral speed is several times that of the stream, introduce an entirely new order of importance into the end effect and therefore into aspect ratio. It would be fortunate if so simple a device could correct the detrimental results so completely, but the additional resistance has yet to be considered.

The American lift curves correspond closely with the German curve for the end shield of double the cylinder diameter, and the latter approaches the results for infinite aspect ratio as far as lift is concerned.

When we come to consider drag, there is a heavy discrepancy between the two reports. The polar curves of the German laboratory show a maximum ratio

of lift to drag of only 3.5, while the American report gives a maximum ratio of about 8.

On the complete model of the " Buckau " the maximum ratio was somewhat less than the value 3.5 obtained for the cylinder.

We may infer that end effect for a cylinder of 4.7 : 1 ratio of length to diameter, takes the form of transverse circulation which more than doubles the drag by increased dissipation, and reduces the useful circulation and lift to less than half by bringing in fresh air at the ends.

End shields of twice the cylinder diameter effectively restore the useful circulation, but scarcely affect the drag, which remains at twice the magnitude of the non-rotating cylinder.

We are now in a position to draw a provisional conclusion. If the best ratio of lift to drag obtainable under practical conditions is only 3.5 it is unlikely that there will be any useful application to aeroplanes, airscrews, or helicopters, which have to make good the losses of energy due to drag by the consumption of petrol. For rotor ships which draw energy from the trade winds the loss is a minor consideration provided it does not impose impracticable size and weight of plant and a poor angle when sailing into the wind. In both these qualities the rotor ship appears to be decisively better than the sailing ship.

If the figure for ratio of lift to drag obtained by the American laboratory were really obtainable in practice, namely, 8 : 1, then it would probably be possible to make rotor aeroplanes which would at least fly.

(To be continued.)

REVIEW

Makers of Science

By Ivor B. Hart. (Clarendon Press. 6s. od. nett.)

Mr. Hart is known favourably as joint author of an elementary text book of aeronautical science.

In the volume under review he brings a zest to his subject which cannot fail to enliven the interest of the general reader.

The introduction by Dr. Charles Singer is in itself a contribution of value to the problem of education, raised by the revolution which has replaced the classics as the staple subjects. He gives warning of the ill done by narrow concentration on equipment for earning a living.

The author in his earlier chapters gives many vivid glimpses of the wise men of the ancient Greeks, that wonderful race which emerges from the gloom of prehistoric times, and after a sojourn, departs into nothingness, leaving behind a bright illumination on every activity of the human mind—a miracle that can never be repeated in all its freshness in our historic times. After the fall of Rome and Constantinople, and the dark ages, we have the birth pangs of the renaissance.

The struggle between the fixed dogma of medieval church and state and the irresistible thrust of the scientific spirit is well told in the sketches of Roger Bacon, Copernicus, Kepler and above all Galileo. The completion of the revolution in philosophy by Descartes and in physical science by Newton are fittingly brought out.

The great scientists of the French revolutionary times are passed over with a too brief reference, and the prominence given to Davy, Faraday and Kelvin, indicate the author's preference for men who apply their scientific knowledge. On the other hand, Stokes is not mentioned and Clerk-Maxwell has but a brief reference. The latter's "Electricity and Magnetism" is only comparable with Newton's "Principia," in that it inaugurated an era which has produced on the one hand modern atomic theories, on the other Einstein's great generalisation of the time-space continuum. Finally, a book which mentions Raleigh might give space for Rayleigh.

As minor blemishes in style might be mentioned the use of *clichés* and even of slang—"England was solid for Newton"—and the too frequent rhetorical question.

Differences of opinion on style and selection are inevitable and do not affect the general impression of fresh and lively interest in the lives of great men of science.

It is to be hoped that this little history will be freely prescribed by teachers of science as a corrective for too narrow specialisation, and that its readers will develop a taste for further reading in the history of science.

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